BOSTON UNIVERSITY
COLLEGE OF LIBERAL ARTS
LIBRARY

THE AMERICAN
JOURNAL OF PSYCHOLOGY

Founded in 1887 by G. STANLEY HALL

VOL. XXXVI OCTOBER, 1925 No. 4

THE ACQUISITION OF ABSOLUTE PITCH*

By HELEN K. MULL, Vassar College

PAGE
History 469
Problems and Observers 477
Results: Group 1 478
Results: Group 2 482
Results: Group 3 487
Introspective results 489
Summary of results 491
Conclusions 492

History of the Problem

The problem of absolute pitch came to psychology from the musical world, where, as early as Mozart's time (b. 1756), the ability to identify notes upon hearing them was recognized among musicians as an unusual gift. It was Stumpf[1] who introduced absolute pitch anecdotally, theoretically, and to some degree experimentally, to psychology, which only within the last few years, however, has brought it into the laboratory in true scientific fashion. Here the work has been chiefly upon individuals who already possess absolute ear, and who, as it turns out, can furnish no introspective clue to the basis of their judgments, which come too easily and immediately for inspection. Neither are the objective results of different *O*s similar or clear-cut enough to point unequivocally toward any particular explanation. This state of affairs turns psychologists again theoryward in their search for an understanding of the nature of absolute judgments, and discussion centres around tonal attributes. There is naturally little agreement as to what attribute, or combination of attributes, is chiefly accountable for such judgments, inasmuch as there is no unanimity of opinion as to what actually are the attributes of tone. Even the meaning of absolute pitch cannot yet be narrowly defined by either an objective or a subjective criterion. For if we say, as Baird does,

*From the Harvard Psychological Laboratory.
[1]C. Stumpf, *Tonpsychologie*, I, 1883, 280, 305ff; II, 1890, 380, 553.

that it is the ability "to identify pitches without having recourse to any process of comparing or relating "[2] (to the pitch of a known standard note), we are still being indefinite upon the matter of the *degree* of ability a person must have in order to qualify. Again, positive definition by a subjective criterion is impossible, since the experimental basis of judgment is unrecognized by those who have absolute ear, and is consequently the subject of most varied speculations on the part of theorists.

In presenting the historical setting of the problem of absolute pitch, it seems advisable to give first place to a discussion of the principal facts which have come out in the course of observation and experiment upon it, and afterward briefly to mention various systematic accounts of the phenomenon.

Degrees of Precision. That there are degrees of precision among the "gifted" individuals, is generally recognized, but it is stated nowhere more convincingly and explicitly than in Baird's[3] work upon 9 such persons. Here are exhibited all degrees from 100% to 0%, both by the group and by individuals, under the various experimental conditions given by different kinds of instruments, different pitch regions, and different notes to be judged. Baird claims that his *O*s possess absolute ear, however, because their averages on each of the instruments are in every case higher than would be due to chance, which he says is the extent to which the normal individual would judge correctly. Boggs[4] also found that the percentage of correct judgments made by her 6 "gifted" *O*s upon piano notes chosen at random from a certain arbitrarily selected number of octaves ranged from 100% to 16%. Both of these scores were made by the same individual.

What degree of precision shall be reached seems, then, to depend both upon the experimental conditions and upon the individual himself. We are now concerned to discover, further, upon just what external and subjective factors absolute ear may rest.

Pitch Region. Typically, the greatest precision occurs in the middle pitches—typically, but not invariably; for Boggs[5] found that pitch region makes very little difference to accuracy in the case of some of her *O*s. Nor does the middle region seem particularly favorable to Meyer[6] and Heyfelder, if we are to judge from the results of the experiments in which they took part. On the other hand, Baird[7] found quite generally that the middle range is easiest to judge. Von Kries[8] also, who has absolute ear, finds this to be true in his own case. He attributes greater precision in the middle region to its greater musical value, and to the fact that pitch discrimination is there finest. Stumpf[9] likewise recognizes this regional superiority, but attributes it rather to greater use and consequent familiarity, discounting fineness of relative pitch discrimination as an explanation, since, for his *O*s, regions of greatest accuracy for absolute pitch were not always coincident with those of greatest accuracy for relative pitch.

---

[2] J. W. Baird, Memory for Absolute Pitch, *Titchener Commemorative Volume*, 1917, 44.

[3] *Ibid.*, 53.

[4] L. P. Boggs, Studies in Absolute Pitch, this JOURNAL, 18, 1907, 194-205.

[5] *Ibid.*, 202.

[6] M. Meyer, Is Memory of Absolute Pitch Capable of Development by Training? *Psychol. Rev.*, 6, 1899, 514-516.

[7] Baird, *op. cit.*, 75.

[8] J. von Kries, Ueber das absolute Gehör, *Ztschr. f. Psychol.*, 3, 1892, 263, 268.

[9] Stumpf, *op. cit.*, I, 313.

Révész[10] notes the greater familiarity of the middle register, but speaks also of a possible fundamental superiority which it may have. Just what he means by "fundamental superiority", or what von Kries means by "musical value", is difficult to say; but the terms seem to emphasize the nature of the stimulus. All of the writers, however, recognize that individual, subjective differences are also of importance; there must be some "individual coefficient", as Stumpf[11] calls it, which enables an individual to respond adequately to the stimulus. Otherwise there would be nothing unusual about absolute ear. We shall see in the systematic accounts what each writer considers the "individual coefficient" to be.

Naturals vs. Accidentals. It is not only pitch region that influences accuracy. According to Baird[12] certain notes, namely "f", "e", and "g", are generally more favorable for absolute judgments, though these notes are not easiest for every one. Generally, also, naturals are better judged than accidentals, though there seems to be no reason for believing in the tradition that each, as a class, has a distinctive character. Boggs,[13] it is true, found one of her *Os* able almost invariably to distinguish naturals from accidentals, but she attributes it to the fact that in this case the naturals had names of their own, whereas the accidentals did not. In the case of the other *Os*, the accidentals had distinctive names as well as the naturals. In her opinion, one essential requirement for absolute pitch is that there shall be invariable association between a note and its name.

As early as Helmholtz[14] we find a discussion of the allied subject of key-character, which Helmholtz himself believes to exist, but not to be a matter of absolute pitch in so far as it depends upon incidental physical characteristics of the stimulus. Incidental characteristics, however, will not explain all cases. The most general explanation Helmholtz offers, and the only one which he considers really to be a matter of absolute pitch, is that the cue may come from g'''', which is the "proper tone of the human ear", and so has a special brilliance. The extent to which this tone is present as an overtone in other notes may be a determining factor, though he does not believe it to be a very important one.[15] This latter explanation might be applied to absolute pitch for single notes, which Helmholtz himself does not discuss, as well as to key-character.

Timbre. We noted, in treating of degrees of precision, that some timbres are more accurately judged than others. It may be that overtones are in some way important *per se* or, on the other hand, it is possible that it is the familiarity of the clang which should receive the emphasis. Or it may be that both factors contribute.

Stumpf[16] admits the importance of timbre and, consistently with his views concerning pitch region, sets some store here also by familiarity. The most familiar timbres are the easiest to judge. But there are other factors as well. "Alles hängt hier von der Übung, vom Gedächtnis, eben damit aber auch von einem individualen Koeffizienten, ab." Von Kries[17] is not entirely in accord with Stumpf; for, though he recognizes the importance of timbre and of the individual coefficient, he minimizes the rôle of frequency. If richness of overtones, which according to von Kries need not be consciously present to the *Os*, is in some way important for accuracy in absolute pitch judgments, then chords ought to be easier to judge than single tones. For von Kries himself they are, especially harmonious chords.

[10]G. Révész, *Zur Grundlegung der Tonpsychologie*, 1913, 101.
[11]Stumpf, *op. cit.*, I, 315.
[12]Baird, *op. cit.*, 61.
[13]Boggs, *op. cit.*, 202.
[14]H. L. F. von Helmholtz, *Tonempfindungen*, 1870, 485-488.
[15]Helmholtz, *op. cit.*, 487.
[16]Stumpf, *op. cit.*, I, 305.
[17]von Kries, *op. cit.*, 261, 264.

Nevertheless, they are by no means as frequently heard as single tones[18]. Boggs[19] also finds her *Os* keen in hearing out overtones, and more adequate to chords than to single tones. But, as von Kries points out, overtones cannot be all-important, since pitches produced by the human voice, rich in overtones, are for him harder to identify than those produced by tuning-forks which are practically lacking in overtones.

Another argument against the sufficiency of an explanation on the basis of familiarity is the fact that there seems to be a determinable order of difficulty among the various instruments. Von Kries[20] finds general agreement among musicians that the order, from least to most difficult, is as follows: piano, strings, wind-instruments, voice, whistling, tuning-forks, and bells. Baird's[21] order is strikingly like that of von Kries: piano, organ (diapason, reed, string, flute), voice, and tuning-forks. Though it is possible that this order of difficulty might be, in general, the order of familiarity as well, neither Baird nor von Kries thinks that familiarity has much to do with the situation, and both cite in support of this view the fact that the voice, which is most familiar, is among the most difficult to identify.[22] The argument seems fallacious, however, because it is not a single human voice which is repeatedly heard, but many different human voices, among which there is much less uniformity than among mechanical musical instruments. Moreover, as Köhler[23] points out, any particular voice is unstable because of vowel differences which considerably affect its tone-body. He finds that persons with absolute ear are inadequate to unfamiliar clangs. And, unlike von Kries' and Boggs' *Os*, his *Os* were not more adequate to chords than to single notes.[24]

Timbre, then, is probably important both *per se* and also by reason of its familiarity to the individual; at least we do not seem justified in leaving out either factor in an explanation of absolute pitch in general. In any individual case a different conclusion might be reached.

Errors. Errors in absolute judgments, we have seen, are made in all degrees, depending upon the individual and upon the conditions under which he is rendering his judgments. But there are certain alleged peculiarities in the nature of errors made by "gifted" individuals which are sometimes considered important. Foremost of these is the fact that a note is often given its proper name, but is assigned to the wrong octave. Baird[25] bases his explanation of absolute ear largely upon these "octave-errors." He discusses the possibility of their occurrence by reason of the extreme community of overtones in notes an octave apart, but rejects it finally, in the face of the fact that the next most frequent errors are thirds and sixths, which do not have as high a degree of community of overtones as fourths and fifths, which come after thirds and sixths in order of frequency. He considers this fact to be evidence which justifies the assumption that there is a certain something, "quality" he calls it, after Révész, which is alike for all "c's", alike for all "d's", but different as between "c" and "d". It is an attribute, "obscurely present" in all tones, but clearly experienced only by those who have the gift of absolute pitch. Such an attribute would explain very neatly the likelihood of one "c" being mistaken for another.

---

[18]von Kries, *op. cit.*, 271.
[19]Boggs, *op. cit.*, 202, 204.
[20]von Kries, *op. cit.*, 264, 272.
[21]Baird, *op. cit.*, 50, 74.
[22]von Kries, *op. cit.*, 275, 276; Baird, *op. cit.*, 52.
[23]W. Köhler, Akustische Untersuchungen, *Ztschr. f. Psychol.*, 72, 1915, 168.
[24]Köhler, *op. cit.*, 168.
[25]Baird, *op. cit.*, 65, 66, 76.

There are, however, other explanations of octave-errors. Stumpf[26] considers them to be due simply to misapprehension or ignorance as to what pitch-regions the different octave designations signify. Accordant with this explanation is Köhler's[27] belief that such errors do not occur on familiar instruments. To Watt[28] they are an indication of the extent to which absolute ear is subordinated to relative ear. The placement of the note within the octave, that is, its interval, is of primary importance, and the placement of the octave a secondary matter. Ogden[29] believes that the reason errors of fourths and fifths are of comparatively infrequent occurrence, as against thirds and sixths, is that they are more prominent divisions of the octave, and so get more attention and individuality. As may be surmised, neither Watt nor Ogden explains absolute pitch without reference to the musical interval, upon which they both base their systems of hearing.

Since it is Baird who places most importance upon the nature of the errors, let us examine his results somewhat more closely. Usually, he finds, errors do not exceed three semitones, and those of one semitone are most frequent. He gives no data to show the relative frequency of octave-errors to the other errors. Errors of thirds and sixths are frequent, but one can not be sure in the case of sixths that the *O* did not mean to place the note, say, in the octave above, and judged it a minor third too low. Again, he gives no data to show the relative frequency of errors of tritones and sevenths. It is also possible that a different statistical treatment of errors might result in a different order of frequency (*cf.* pp. 484f).

Kinaesthesis and Imagery. If one were not disposed to call in a special attribute of tone, which only those persons gifted with absolute ear could clearly perceive, one might think first of kinaesthesis as a possible explanation of the phenomenon. Absolute judgments might be based simply upon the remembrance of the vocimotor feeling of the various notes, or of the feeling of one note, to which all other notes are related by interval. It is hard to disprove the sufficiency of such an explanation, especially when one remembers the "implicit" responses urged by the behaviorists. But if kinaesthesis is important, its influence is, to say the least, often very obscure and unconvincing. With regard to the relating of all notes to one note, there is ample testimony that there are many persons, who possess absolute ear, who do not have good sense of interval.[30] And with regard to the possibility of each note having a different feeling, it is not clear why kinaesthesis should take precedence over auditory sensations. But let us examine more closly the evidence for and against the importance of kinaesthesis in absolute judgments.

There are many cases where recognition of a given pitch is possible without reproduction of the same pitch when it is designated by name.[31] This fact certainly obscures the rôle of kinaesthesis, though it proves nothing against it; for recognition is always an earlier stage in association than recall. Again, von Kries[32] affirms that he can judge female voices, whose pitches he cannot reproduce, better than male ones, and that he can never estimate his own vocal or whistling tones correctly. A third argument against kinaesthesis is the fact that absolute ear often dates from childhood, and children's voices change as they grow older, although it is possible that readjustments might occur. A more conclusive argument lies in the fact that some individuals can judge notes correctly without

[26]Stumpf, *op. cit.*, I, 311.
[27]Köhler, *op. cit.*, 172.
[28]H. J. Watt, *The Psychology of Sound*, 1917, 85.
[29]R. M. Ogden, *Hearing*, 1924, 163, 164.
[30]Köhler, *op. cit.*, 160; Stumpf, *op. cit.*, I, 269; von Kries, *op. cit.*, 276.
[31]Köhler, *op. cit.*, 174.
[32]von Kries, *op. cit.*, 271, footnote 2.

being able to carry a tune, to reproduce a given pitch, or even to misrepresent it consistently. Nevertheless, this argument is not entirely conclusive, for there may still be some consistent kinaesthesis adequate to the single note. Granting this, we have still to account for the undeniable fact that some timbres are more easily judged than others.

When we turn to the introspective reports, we have Stumpf's[33] evidence that the transposition of a tone to the middle register sometimes occurs to advantage. Boggs[34] found that there may be various accompaniments, such as humming and visual or motor imagery. But she also finds that there is a positive correlation between immediateness and accuracy of judgment and that, typically, accurate judgments are immediate both in time and in experience. Baird[35] found that, though humming is sometimes successfully employed, nevertheless, in test series in which his *Os* were given all the time they wanted to make their judgments, a smaller degree of accuracy was shown than in those experiments in which only sufficient time for immediate judgments was allowed. Like Boggs, he found that, even without the necessity imposed by time, judgments were immediate.

This is enough evidence to show that, although kinaesthetic sensations and imagery of various kinds have been known to occur in judgments of absolute pitch, accompanied sometimes by success and sometimes by failure, they are usually lacking. What, now, can be the basis of the typical judgment? Has it anything to do with musical ability?

Musical Ability. Discussion on the preceding topics has indicated that good sense of interval, fine pitch discrimination, and ability to carry a tune are not necessary accompaniments of absolute ear. Stumpf[36], however, considers interval-sense to be the only dispensable musical ability. Ogden[37] and Boggs[38] also think it likely that all persons "gifted" with absolute ear are also gifted with musical ability. On the other hand, Köhler, von Kries, and Watt emphasize rather its unimportance, and von Hornbostel states that it is not an invariable accompaniment. We can hardly estimate the musical talent of parrots and starlings, but it is not likely to be as highly developed as it is in human beings. They might be said, however, to possess absolute pitch, since they repeat melodies only in the keys in which they were learned.[39]

The foregoing facts which have been ascertained with respect to absolute pitch leave us chiefly with a realization of their contrariness. Conclusions drawn from them can be only of the most general kind. It does seem, however, that (1) something depends on the stimulus, and that (2) something depends on the individual.

To account more explicitly for the phenomenon displayed, two general explanations have already been indicated: (1) the individual may possess a special ability, differing in kind from anything possessed by the average person; or (2) he may possess merely a high degree of some ability which is found to some extent in all persons.

The most unconditioned exponents of the first explanation are Boggs, Révész, and Baird. They believe that quality is the distinguishing feature of a tone in absolute pitch judgments. Boggs[40] was at first inclined to think that ability to hear out overtones was the cause of the absolute ear, but later changed her view from timbre to quality. Quality, she holds, enables

[33]Stumpf, *op. cit.*, I, 308ff.
[34]Boggs, *op. cit.*, 204.
[35]Baird, *op. cit.*, 69, 72.
[36]Stumpf, *op. cit.*, I, 289, 306ff.
[37]Ogden, *op. cit.*, 161.
[38]Boggs, *op. cit.*, 204.
[39]E. von Hornbostel, Ueber vergleichende akustische und musikpsychologische Untersuchungen, *Ztschr. f. angew. Psychol.*, 3, 1910, 465-487.
[40]Boggs, *op. cit.*, 202-204.

a person to hear tones in much the same way as those with color vision see colors.

Baird's[41] view is similar. He bases it chiefly on octave-errors, as we have seen, and on immediateness of judgment, which he thinks did not become immediate by practice, but was originally immediate, like the recognition of red when once it has been pointed out as red.

Révész[42] makes a definite division between those who recognize tones by pitch, and those who recognize them by quality. Both have genuine absolute ear, but it is the latter group whose judgments are most immediate and most correct. Ability to distinguish quality is innate, like color-vision, and cannot be acquired. Pitch-memory, on the other hand, may be acquired and perfected by diligent practice. Those who have quality usually have pitch also, and the two work in conjunction, so that the octave-errors are comparatively infrequent and can soon be eliminated by regional pitch-memory. Qualitative memory may be present for certain regions only. To explain the influence of timbre, he says it is possible that, when other parts of a tonal complex are changed, the quality cannot be recognized in the new combination.

Révész' position is based largely upon the results of his investigations on an abnormal *O*, von Liebermann, for whom "pitches" were abnormal, so that he could not judge intervals correctly, while "qualities" of the component notes remained unchanged. Von Liebermann's case allows, however, of other interpretations. Ogden[43] attributes his confusion of the fifth with the octave, for instance, where the judgment of their "qualities" remained intact, to "a reduction of von Liebermann's musical ability to that of an unmusical person." The defect is an integrative one, of central origin presumably, since the disturbances were binaural.

Stumpf and von Kries hold similar positions, though that of Stumpf seems less extreme. Both speak of *Anlage*, which they do not further define. For von Kries[44], however, it is something which cannot be acquired, while Stumpf leaves the question more open, and gives more weight to familiarity, musical ability, feeling, and interest, as displayed in practical musical activities.

It is almost impossible to disprove theories of special ability, but it is certainly undesirable to multiply entities unnecessarily. Rich[45], in 1919, made an experimental study of tonal attributes by a method of independent variability. He finds some evidence for an attribute of "tonality," defined as "that attribute by which (musical) tones are named." His *O*s, it is true, had difficulty in knowing what to observe when asked to observe tonality; but that proves nothing against the tonality (quality) which we have been discussing, since the *O*s, who were not gifted with absolute pitch, would not be expected to observe it easily. But Rich found that the DL for what his *O*s took to be tonality increases steadily without following Weber's Law. Just why the middle pitch-region should be easiest to identify in absolute pitch is not explainable by this fact, if the *O*s in his experiments were judging quality, and if judgments of absolute pitch rest upon the same attribute.

The most convincing proof of the spuriousness of tonality, in Baird's sense of the term, would be success in training average individuals in absolute pitch to the point where they were similar to the "gifted" individuals in every respect.

---

[41]Baird, *op. cit.*, 77, 78.
[42]Révész, *op. cit.*, 90-101.
[43]Ogden, *op. cit.*, 161.
[44]von Kries, *op. cit.*, 261, 262.
[45]G. J. Rich, A Study of Tonal Attributes, this JOURNAL, 30, 1919, 121-164.

Explanations of absolute pitch which do not rest upon a special ability are held by Meyer, Köhler, Watt, and Ogden.

Meyer[46] holds that every one has absolute ear to some extent,—that there is no sharp line of distinction, which, if it existed, would indicate a physiological difference between normal and "gifted" persons. He considers some features of the tone-body, rather than pitch, to be the basis of absolute judgments.

The theory that tone-body, and not pitch, is important, has been developed by Köhler.[47] Tone-body includes brightness, vocality, volume, and intensity. It is recognized immediately, that is, without the intervention of a memory image. Experiments on himself, in which he tried to learn the white notes between C and h''', showed a great improvement in correct pitch-naming after he changed from a pitch *Aufgabe* to one of tone-body. In judging upon this latter basis he found himself relying upon gross brightness for regional placement and, further, upon a characteristic which was different for each note. Some notes were "geschlossen", others "zwiespaltig", and so on. These features of tone-body were not present in pure tones, and pure tones are notably difficult to place. (On an attributive explanation it would be difficult to explain why pure tones should offer any special difficulty, since they have the same attributes as those presented by complex ones.) For unfamiliar timbres, recognition is quite impossible, Köhler holds. He explains his errors of fourths and fifths by claiming that, when attention is upon tone-body, it is away from pitch, and if two pitches, not too different, have similar tone-bodies, one may quite easily be taken for the other.

Watt[48] disagrees with Köhler's list of attributes and considers that he has merely shown that timbre is important. Watt believes the attributes of tone to be two, pitch and volume; pitch is ordinal, volume extensive. Both are taken into account by absolute ear, where there may be either a special refinement of sensory apparatus or a better auditory memory. But he thinks that in many cases of absolute pitch there is required also an absolute point of reference in auditory orders (pitches). The problem is analogous to that of localization on the skin, where, however, the great influence of relativity does not exist. It is due to this lack of relativity that tactual localization is possible for all persons. Auditory localization is possible only to a few who have not allowed the relative ear to dominate the absolute ear. According to this view, we all have had a chance to possess absolute pitch, but because of our education in musical relationships we have lost it through disuse. We do recognize noises and the sounds of our friends' voices, where the influence of musical relationships does not impose itself.

Ogden, who, as we noted, bases absolute ear upon a refined sensitivity to different clang-patterns, also insists upon the necessity of reference to a scale with which the individual who possesses the talent is familiar. "The tone must find its place within the setting of an octave and thus establish its interval before its pitch can be named."[49] This explanation is essentially like Watt's. With Ogden, however, the point of reference seems to be introduced for purely systematic reasons and to be considerably overemphasized, especially when one remembers that good interval-sense is not a requisite for absolute pitch and that relationships are generally considered to be a hindrance rather than a help. A note must have a name, and the name is likely to imply musical relationships; but unless the relationships are properly comprehended and help to make the perception richer or more definite, they seem gratuitous.

---

[46]Meyer, *op. cit.*, 514; also Köhler, *op. cit.*, 173, quoting Meyer.
[47]Köhler, *op. cit.*, 161ff.
[48]Watt, *op. cit.*, 85, 263ff.
[49]Ogden, *op. cit.*, 163.

Problem and Observers

Problem. From the foregoing discussion, it seems that there is little reason for calling in a special attribute of tone to explain absolute pitch. There would be even less reason if average individuals could be trained in absolute pitch until they were just like those "gifted" with it. The following experiments were undertaken to test this possibility, and to discover, if possible, the basis of absolute judgment. The results point to still another explanation than any so far given, —to one which stresses the importance of attention. Stumpf hints at it when he speaks of interest and practical musical ability. Boggs also implies it when she remarks that there is great need for concentration in judging pitches absolutely, as is shown by requests for repetition of the notes, and by the distraction caused by outside disturbances. Ogden thinks the most favorable clangs are "striking and uniform." Köhler too notes that one direction of attention is more effective than another, but his emphasis is chiefly upon that which is attended to. What justification we find for emphasis upon attention itself will be discussed in connection with the results.

Observers. There were 12 *O*s in these experiments. Miss E. W. Amen (A), Mr. A. M. Brues (B), Mr. J. G. Beebe-Center (B-C), Mr. L. R. Frazier (F), Mr. W. S. Hulin (H), Mr. J. M. McGinnis (McG), Mr. K. E. Zener (Z), and the writer (Mu), were graduate students in psychology at Harvard University. Dr. A. A. Roback (R) and Dr. M. B. Pratt (P) had the doctorate from Harvard and Clark Universities respectively. Mrs. E. C. Gilbert (G) and Miss K. Miller (Mi) were college seniors 'majoring' in psychology. These particular individuals were selected in order to make up a group representing various degrees of musical ability and training.

Preliminary tests soon made it apparent that none of the *O*s was "naturally" gifted with absolute ear. Three, B, R, and McG, had never had any muscial training outside of that afforded by secondary schools. Moreover, these three *O*s had the greatest difficulty in reproducing vocally a given pitch that was well within their vocal compass, though one of them (R) had no difficulty whatever in carrying a tune. A and G had had only a few piano lessons, but did not "keep up" their music. The others had had considerable training, and still engaged in some sort of musical activity. The Seashore tests of musical ability show Mi, B, Mu, F, P, and Z, as ranking in the first quartile in the order given; H, G, and R, in the second; and A, C, and McG, in the third.

General Procedure. The experiments and the *O*s fall into three groups, according to the phase of the problem with which they are particularly concerned.

The first group dealt with the acquisition and perfection of absolute ear for one note. The *O*s were trained with respect to one note only, in the belief that a greater though limited training would yield more rapid and more precise results than the casual acquaintance with many notes. The assumption was that, if one note could be learned, all could be learned, in time. In this group the problem of the transfer of the acquired ability to some instruments other than the one learned also arose.

In the second group, the ability to judge notes correctly, before special training, was investigated more thoroughly than in the case of the first group. The rate of forgetting after special training, and the correlation between speed and accuracy of judgment, were also determined.

The third group of experiments dealt with the acquisition of absolute ear for all the notes of the chromatic scale of an octave in the middle pitch-region.

Results: Group 1

Procedure. Six persons, A, B-C, F, H, Mi, and R, took part in the first group of experiments. Each observed one hour a week throughout most of the college year, excepting that A and R came once a week through the first semester, and twice a week during the second semester.

Preliminary tests, consisting in the presentation of various single notes of the diatonic scale, selected from three octaves in the middle pitch-region, were made to make sure that the *O*s were not already possessed of absolute ear. After it was sufficiently demonstrated that none of them was so "gifted", intensive training was begun.

The Appunn tonometer was used throughout all of the experiments, excepting that in the first few, when the tonometer was not available, the Ellis harmonical was used.

With *O*s B-C, H, and Mi, middle c (c′; 264 d. v.), the note selected to be learned by this group, was sounded once every minute for 15 min. Each presentation lasted 5 sec. To the other three *O*s, A, F, and R, it was sounded continuously for 15 min. In these trials attention was called to the note once every minute for five sec., and between attentive periods the *O*s were instructed to distract themselves by reading or studying. These two modes of training were adopted in order to discover whether a stimulus unattended to would be of advantage in the acquisition of absolute pitch. During the 5 sec. attentive periods, all the *O*s were allowed to sing the note, or to "do" anything else they pleased with it; and they were encouraged to observe everything they could about the note. Introspections were required at the end of the 15 min. Then, after a short but effectual period of auditory distraction, which pre-

vented the continuance of after-images, the test experiments were begun.

In the test experiments the *O*, seated with his back to the instrument, was presented with a series of notes chosen from among the naturals from c to b′ inclusive. The possibility of melodic clues was avoided by making the series as musically nonsensical as possible; by using, in any series, only some of the notes; and by the occasional introduction of accidentals in extreme regions. The *O* was to respond by saying "yes" when he judged the note presented to be c′, and "no" when he judged it to be a "not c" note. Any note was repeated upon request, and requests turned out to be frequent, for outside disturbance was very distracting. The series ended automatically with a "yes" judgment. Then the *O* was told whether he had judged correctly or not and, if not, what the note was, in order that he might know the direction and magnitude of his error. Then c′ was presented again for 5 sec. to offset the tendency to form habitual errors. There were usually 20 of these test series given at one sitting, with auditory distraction between them. Free introspections as to the basis of judgment were called for, often after single judgments, and sometimes at the end of an hour.

After a month, other experiments were begun, in which the notes used were much closer together than those of our musical scale. Nine notes, including the c′ already being learned (264 d. v.), separated by equal increments of 8 vibrations, were selected between 232 and 296 d. v. inclusive, and were presented in test experiments of the same nature as the previous ones, except that this time the series did not end with the giving of a "yes" judgment: all 9 notes were presented, in haphazard order, in every series. The *O*s were told, however, that c′ might be presented more than once in a series, or that it might not be presented at all. This method has the advantage of presenting all the notes an equal number of times, and thus facilitating computation of the results.

Experiments of these two sorts were made alternately every two weeks. Toward the end of the year, tests of the first kind were made with three different organ stops (diapason, string, and flute) to study transfer of learning.

Amount of Learning. If, for purposes of comparison, we select from the pre-training records of all the *O*s only those cases where c′ was presented and judged, we find that judgments were correct only 40.4% of the time, and that in 59.6% of the cases c′ was mistaken for a note not c′. The average error computed from all the notes that were judged as c′ is 285 cents (100 cents = 1 semitone). The untrained *O* was correct with respect to c′ less than half the time, and made on the average an error between a musical second and a musical third. The final records

show that judgments were correct 82% of the time, that is, that in only 18% of the cases was c′ mistaken for a note not c′; the average error was reduced to 33 cents. These averages are representative of the individual records (see Table 1).

There was, then, decided improvement; but can we say that, at the end of their training, the *O*s had acquired absolute ear for middle c? Baird's[50] figures show an average error of 24.5% (as against our 18%) with the most favorable stimulus, piano notes; and of 37.9% with organ notes. The magnitude of individual errors of 4 of his 9 *O*s was rarely greater than 3 semitones. The same thing may be said of 5 of our 6 *O*s, as can be seen from Table 1. It thus appears that our *O*s acquired absolute ear for middle c to the degree that Baird's *O*s had it.

Importance of Marginal Stimulus. It will be recalled that to three of the *O*s of this group, B-C, H, and Mi, c′ was presented intermittently and only under attention; and that to the other three, A, F, and R, it was presented continuously, but part of the time under distraction. The duration of attention was alike for both groups. A scrutiny of the individual records reveals no striking superiority of the second over the first group. The records of the first are, if anything, superior; individual differences, however, seem to be the potent factor rather than the difference in methods of training.

Rate of Learning. By far the greatest amount of learning occurs in the very beginning, during the period of intensive training preceding the first test experiments. The latter show an average of 70% correct judgments, with an error of 84 cents, as against the 40% and 283 cents of the pre-training experiments. Table 2 shows that there is on the average a steady improvement, although as can be seen from Table 1 there are irregular fluctuations in the individual records.

Transfer. Table 2 shows that there was a high degree of transfer of learning to notes of the diapason, string, and flute organ stops. As contrasted with the 40.4% correct judgments of the pre-training records, diapason notes show an average of 76.3%; string notes, of 77.1%; and flute notes, of 77.7%. From Baird's experiments we have these corresponding figures; diapason, 59.2%; string, 52.4%; flute, 49.7%. The order appears to be opposite in the two cases; but when we remember that the flute errors amounted to 42 cents in the average, and the diapason and string errors to 25 and 24 cents, respectively, a modification of the order indicated by the percentages above seems advisable, especially since it is not representative of the individual cases. Two of the *O*s made the highest percentages of correct judgments upon diapason notes, two upon string, and two upon flute notes. A slightly more representative order is

[50]Baird, *op. cit.*, 57, 59, 61.

TABLE 2

	Av. percent. of correct "c" or "not c" judgments	Av. error (cents)
Before training	.404	283
Test 1	.700	84
" 2	.743	61
" 3	.694	58
" 4	.714	48
Final tests	.820	33
String	.771	24
Diapason	.763	25
Flute	.777	42

given by the average errors, where we find that the flute errors are greatest in three cases, string errors in one case, and diapason errors in one case. It does not seem reasonable to attach much importance to the difference of our percentage order from Baird's, which also, like ours, is not completely representative of the individual cases.

There is the possibility that by the time these new stops were used, the *O*s' general method of judging had become perfected; so that, after the new c′ was once heard, they knew how to recognize it subsequently. This factor may indeed account partially for the results; for, as we have already noted, learning is fairly rapid; but transfer is the only explanation of the fact that, of 24 initial presentations of new c's, 16 were judged correcly, or in other words that 16 c's out of 24 were judged correctly the very first time they were heard.

Magnitude and Direction of Errors. Baird states that the errors made in judgments of absolute pitch seldom exceeded three semitones except in the case of 4 of his 9 *O*s. Table 1 shows that this is true of our *O*s also, with the exceptions B-C and R.

The direction of our errors is chiefly that of overestimation of c′, both for the tonometer notes and for the organ stops; but with respect to the notes presented, the terms in which Baird speaks, the errors are underestimations. The average error calculated from the notes below c′ that were judged as c′ is 20 cents for tonometer notes, and 11 cents for organ notes; whereas

[51]The average errors in cents are calculated on a basis of the number of judgments made in any test, instead of upon the number of times the notes were presented, as is done with the percentages in Table 1. It is difficult to decide which figures to use when comparing our results with Baird's. In his case the two methods would have yielded identical results, since every note that was presented was judged by its own name. Our "not c" judgments of c′ introduce errors of omission taken into account by the percentages but not by the errors in cents. The latter, however, are the best single estimate of performance.

the error calculated from the notes above c′ mistaken for c′ is 61 cents for tonometer notes and 16 cents for organ notes. Baird[52] also finds that with organ notes underestimation is relatively more frequent.

Results of Experiments with Notes of 8 d. v. Differences. The results of these experiments in all essential respects confirm those already discussed, as Tables 3 and 4 will show, except for the fact that in this case we have no record of the *O*s' ability previous to training; when these experiments were begun, c′ was already well established.

From an average of 43% of c′ presentations correctly judged, with an average error of 68 cents in test 1, the *O*s attained to 57% correct judgments in the final performance, with an average error of 29 cents. There is average improvement from test to test in respect of the diminution in size and number of errors. In respect of the percentage of presentations correctly judged, there is one inversion.

The individual records naturally show greater irregularity than the average. It is interesting, however, to note that here, where the task is harder and the percentage of correct judgments is less than in the experiments with notes of the diatonic scale, the individual errors are of less magnitude, that is, they are grouped more closely around c′.

From the total number of errors made upon each of the various vibration rates, an approximately normal curve could be drawn, though it would be somewhat skewed in the direction of pitches above c′, indicating that, as in the case of the diatonic notes, the pitch of c′ is usually overestimated. The average error of overestimation is 31 cents, whereas that of underestimation is 15 cents.

TABLE 4

	Average percent. of correct judgments of 8 d. v. differences	Average error
Test 1	.43	68
" 2	.47	53
" 3	.64	39
" 4	.57	29

Results: Group 2

Procedure. The results of the first group of experiments—particularly the immediateness of learning, the irregularity in individual records, and the unimportance of the marginal stimulus—suggest that an important factor in the acquisition of absolute ear may be simply the giving of attention to the task

[52]Baird, *op. cit.*, 61.

in hand, that is to say, interesting oneself in the note for its own sake rather than for its melodic or harmonic significance. Accordingly, in a second group of experiments, we made attention the variable factor, by putting 4 new *O*s, B, G, McG, and Z, through two series previous to any training, of which the first was just like the pre-training experiments of Group 1, where the various notes presented were to be judged by their own names; and the second consisted in running through the same experiments once more with the *O*s asked to judge merely whether the note presented was "b" or "not b" (the b below middle c, 248 d. v.). We had it in mind to discover whether the heightened attention which a narrowed *Aufgabe* insures would influence the results.

After these two sets of pre-training experiments were completed, the *O*s were trained intensively to b by being asked to listen attentively to it for 5 sec. once every minute for 10 min. Thereafter tests were given at the end of 3 min., 1 hour, 1 day, 1 week, and 1 month, to determine the persistence of learning. These test experiments consisted of the presentation of the diatonic notes from f to e′ inclusive (176-330 d. v.), and in addition two accidentals in a non-critical region (a′♭ and b′♭) to offset the possibility of melodic cues. The series, in each of which all the notes were presented (as in the experiments of Group 1 with a difference of 8 d. v.), were constructed so as to insure their being as melodically nonsensical as possible. Reaction times were taken to discover whether any relation existed between speed and accuracy of judgment.

Pre-training Experiments: (*a*) *Degree of Precision* (*Set 1*). The purposes of this group of experiments were to discover the influence of heightened attention upon absolute pitch, the amount of forgetting, and the relationship between speed and accuracy of judgment. The pre-training experiments, moreover, proved interesting for comparison with Baird's results in certain other respects.

For the group as a whole, the order of correctness for the various notes, as shown by their percentages of presentations correctly judged, is as follows:

g	c′	a	f′	b[53]	d′	e′
.43	.38	.38	.21	.19	.18	.10

Chance would give .11, since the 7 diatonic notes *plus* 2 accidentals were used. We must realize, however, that if all the

[53]This figure is higher than that shown in Table 6 because in this case neither the octave-errors nor the "b" judgments of b′♭ were counted as errors (*cf.* Baird, *op. cit.*, 49).

judgments made had been, for instance, "c" judgments, c′ would have been judged correctly 100% of the number of times it was presented, and obviously this figure would misrepresent the actual state of affairs. Our *O*s did not use such a method, that is, they did not respond invariably with the judgment "c", or "d", or any other one note, but as a matter of fact a comparatively large proportion of their judgments were "c" judgments. The order of frequency of judgment in terms of the various notes, regardless of octave, is:

c	a	f,g	e	b	d	
83	59	57	43	34	31	judgments

The positive correlation between the two orders is .81. If we now look for an order of correctness by getting the percentage of correct judgments for the various notes, on the basis of the number of judgments made in terms of each note, we obtain the following:

b	c	f	d	g,a	e
.47	.36	.30	.25	.19	.09

(The figure of b is high chiefly because of one individual's score.) The notes c and f, then, are both frequently judged and frequently judged correctly, though on the whole the positive correlation between frequency and correctness is insignificant (.009).

No matter which order is considered, it is apparent that, even before training, the *O*s give correct judgments in a greater percentage of cases than would be due to chance. It is also noteworthy that judgments in terms of accidentals were almost never given, even though the *O*s were told that some accidentals would be presented. The *O*s thought in terms of naturals, and in terms of some naturals more than others.

For Baird's *O*s the case is similar: c, f, and g are the notes best judged. He claims, however, that "c-ness", "f-ness", and "g-ness" are peculiarly easy to detect, whereas it is possible that these notes had merely received more attention and emphasis in the course of the *O*'s previous casual experience.

(*b*) *Errors.* Another very interesting phenomenon as shown by this first set of pre-training experiments is the occurrence of octave-errors. Baird considers these errors peculiar to the absolute ear; but, as we have seen (p. 473), they are sometimes thought to be due simply to unfamiliarity with musical instruments. Altogether, there were 14 octave-errors in the present experiments, and 13 cases of errors of more than one octave.

TABLE I

Percent. of number of times presented that the various diatonic notes were judged as e′ at different stages of learning

	f	g	a	b	e′	d′	e′	f′		Av. error[11] in cents below e′	Av. error[11] in cents above e′
Observer A											
Before training					.50	.333		.50		0	175
Test 1					.80	.333	.076			0	67
" 2			.10	.111	.736	.142	.142	.083		20	65
" 3					.944	.23				0	30
" 4				.20	1.00	.25				5	30
Final test			.111		.947	.076				15	10
String					.863	.066				0	10
Diapason					.80					0	0
Flute					.72			.25		0	50
Observer B-C											
Before training: no record, as B-C did not know the notes by name											
Test 1				.40	.388	.35				22	77
" 2					.70	.333				0	60
" 3			.333	.066	.217	.230	.071	.153		68	136
" 4			.125	.222	.235	.487	.20			25	150
Final test				.444	.375	.150				40	30
String				.105	.366	.162				10	60
Diapason				.461	.538					30	0
Flute					.80	.285				0	40
Observer H											
Before training	1.00					.50				250	100
Test 1			.071	.166	.769	.190	.10			17	42
" 2				.10	.736	.312				5	50
" 3					.818	.125				0	20
" 4				.142	.947	.083				5	10
Final test				.125	1.00					5	0
String				.125	.863					5	0
Diapason				.25	.944	.166				10	10
Flute				.222	.941	.181				10	20
Observer F											
Before training	.333					.50		.50		175	400
Test 1				.40	.708	.222	.071			21	42
" 2	.20		.125	.111	.842	.071				55	10
" 3				.142	.95					5	0
" 4				.285	.772	.055				10	10
Final test					.826	.062				0	10
String					.869					0	0
Diapason					.904	.125				0	10
Flute					.689					0	0
Observer Mi											
Before training					.80	1.00				0	66
Test 1					.833	.444				0	57
" 2					.782	.142				0	20
" 3				.166	.772	.066				10	10
" 4					.80					0	0
Final test					.90	.153				0	20
String					.952					0	0
Diapason				.142	.818	.10				5	10
Flute				.10	.782	.083				5	10
Observer R											
Before training					.333			.50		0	250
Test 1			.272	.300	.705	.375	.20	.083	.125	42	114
" 2			.111	.10	.666	.133	.20			20	60
" 3			.076	.062	.466	.136	.055			20	50
" 4				.062	.533	.10	.058			5	40
Final test			.125	.142	.875	.272	.142			20	50
String			.375	.10	.714	.076				50	10
Diapason				.571	.576	.375	.142			26	53
Flute		.333	.571	.125	.733	.272				90	30
Average	.010	.025	.045	.112	.708	.183	.026	.040	.002	16	39

[11] *See footnote on page 481.*

TABLE Ia

ators show the actual number of times the note was judged as c′ at different
arning: denominstors show the actual number of times the note was presented.
ons are reduced to decimals for convenience of inspection in Table I.)

Observer A

f	g	a	b	c′	d′	e′	f′	g
ning				2/4	1/3		1/2	
				20/25	7/21		1/13	
		1/10	1/9	14/19	2/14	1/7	1/12	
				17/18	3/13			
			1/5	16/16	3/12			
		1/9		18/19	1/13			
				19/23	1/15			
				20/25				
				18/25			2/8	

Observer B-C

ining: no record as B-C did not know the notes by name

			4/10	7/18	7/20			
				7/10	3/9			
		4/12	1/15	5/23	6/26	1/14	2/13	
		1/8	2/9	4/17	11/23	2/10		
			8/18	9/24	3/20			
			2/10	12/33	6/37			
			6/13	14/26				
				16/20	4/14			

Observer R

ining					1/2			
1/1		1/14	2/12	20/26	4/21	1/10		
			1/10	14/19	5/16			
				18/22	2/16			
			1/7	18/19	1/12			
			1/8	19/19				
			1/8	19/22				
			2/8	17/18	1/6			
			2/9	16/17	2/11			

Observer F

aining					1/2		1/2	
1/3			5/13	17/24	4/13	1/14		
1/5		1/8	1/9	16/19	1/14			
			1/7	19/20				
			2/7	17/22	1/16			
t				19/23	1/16			
				20/23				
n				19/21	1/6			
				20/29				

Observer M₁

raining				4/5	2/2			
				20/24	8/16			
				18/23	2/14			
			2/12	17/22	1/15			
				20/25				
st				18/20	2/13			
				20/21				
n			1/7	18/22	1/10			
			1/10	18/25	1/12			

Observer R

raining					1/3			1/2	
		3/11	3/10	12/17	6/16	2/10	1/12		1/8
		1/9	1/10	14/21	2/15	2/10			
		1/13	1/16	14/30	3/22	1/18			
			1/16	16/30	2/20	1/17			
st		1/6	1/7	14/16	3/11	1/7			
		3/8	1/10	15/21	1/3				
on			8/14	15/26	6/16	1/7			
	1/3	4/7	1/8	11/15	3/11				

T

Percent. of number of times p
were judged as c′ at different sta

O

D. v. Cents	232 58.70	240 56.76	248 54.45	256 52.43	26 5
Test 1	.10	.30	.20	.20	.4
" 2			.10		.0
" 3			.05	.30	.3
" 4				.15	.2

Ob

Test 1		.40	.10	.30	
" 2			.20	.40	
" 3			.05	.05	
" 4			.15	.10	

O

Test 1	.10		.30	.30	
" 2		.10	.10	.10	
" 3			.10	.15	
" 4					

O

Test 1				.40	
" 2			.10	.30	
" 3		.05	.10	.25	
" 4				.05	

O

Test 1			.20	.20	
" 2					
" 3					
" 4					

O

Test 1			.05		
" 2	.10			.05	
" 3					
" 4					
Average	.012	.035	.075	.200	

TABLE 3

Percent. of number of times presented that the various vibration rates were judged as c′ at different stages of learning

Observer A

D. v. Cents		232 58.70	240 56.76	248 54.45	256 52.43	264 50.94	272 50.19	280 48.77	288 47.44	296	Av. error below c′	Av. error in cents above c′
Test	1	.10	.30	.20	.20	.40	.50	.40			49	32
"	2			.10		.60	.70	.50	.20	.10	5	58
"	3			.05	.30	.55	.50	.25	.15		12	41
"	4				.15	.35	.50	.15	.15		6	47

Observer B-C

Test	1		.40	.10	.30	.20	.50	.40	.20	.20	40	59
"	2			.20	.40	.00	.50	.10			36	30
"	3			.05	.05	.65	.10	.20			8	24
"	4			.15	.10	.40	.35	.15			19	29

Observer F

Test	1	.10		.30	.30	.30	.30				54	12
"	2		.10	.10	.10	.40	.20	.30			27	34
"	3			.10	.15	.55	.20	.20	.05		15	31
"	4					.60	.05				0	4

Observer H

Test	1				.40	.60	.80	.40			10	37
"	2			.10	.30	.70	.30	.10			18	17
"	3		.05	.10	.25	.60	.20	.25	.05		21	29
"	4				.05	.85	.40	.05			27	19

Observer Mi

Test	1			.20	.20	.40	.40	.10	.10		23	33
"	2					.50		.30			0	38
"	3					.80	.20				0	15
"	4					.60					0	31

Observer R

Test	1			.05		.65	.30	.55	.10		3	53
"	2	.10			.05	.60	.30	.35	.05		17	41
"	3					.70	.15	.30	.05	.05	0	43
"	4					.60	.05	.25			0	31
Average		.012	.035	.075	.200	.483	.312	.220	.045	.014	16	31

imes the note was judged as
ors show the actual number
ions are reduced to decimals

272	280	288	296
50.19	48.77	47.44	
5/10	4/10		
7/10	5/10	2/10	1/10
0/20	5/20	3/20	
0/20	3/20	3/20	
5/10	4/10	2/10	2/10
5/10	1/10		
2/20	4/20		
7/20	3/20		
3/10			
2/10	3/10		
4/20	4/20	1/20	
2/10			
8/10	4/10		
3/10	1/20		
4/20	5/20	1/20	
8/20	1/20		
4/10	1/10	1/10	
	3/10		
2/10			
5/20	11/20	2/20	
5/20	7/20	1/20	
4/20	6/20	1/20	1/20
1/20	5/20		

TABLE 5

Percent. of number of times presented that the various notes we judged as b, at various intervals after intensive training. (a′♭ and b were never judged as b.)

	f	g	a	b	c′	d′	e′	f′	Error in cent below b	above
Observer B										
Before training:										
Set 1			.10			.20	.10		50	2
" 2			.10		.10	.10	.30	.30	22	4
3 min. after tr.		.05	.10	.40	.25				50	
1 hr. " "			.10	.50	.30	.05			21	
1 da.. " "			.10	.50	.25		.05		22	
1 mo. " "		.05	.10	.60	.10				47	
Observer G										
Before training:										
Set 1			.10	.10					100	
" 2				.50	.20	.40	.10		0	15
3 min. after tr.			.15	.40	.30	.20			28	8
1 hr. " "		.05	.25	.40	.20	.05			73	6
1 da. " "		.05	.20	.50	.25	.55			52	6
1 wk. " "		.05	.15	.45	.25	.10			50	5
1 mo. " "			.05	.40	.10				18	1
Observer McG										
Before training:										
Set 1		.30				.10			300	7
" 2	.10	.30	.40	.60	.70	.30			108	6
3 min. after tr.		.15	.20	.40	.50	.25			66	8
1 hr. " "		.15	.20	.43	.15	.15			100	5
1 da. " "		.15	.15	.60	.35	.35			56	8
1 wk. " "		.10	.25	.25	.40	.10			81	6
1 mo. " "				.55	.10	.30			0	10
Observer Z										
Before training:										
Set 1	.05	.05	.40	.10					240	
" 2		.10	.30	.35	.155				111	1
3 min. after tr.			.05	.50	.15				14	2
1 hr. " "				.60	.35				0	3
1 da. " "			.05	.50	.30				11	3
1 wk. " "				.65	.25				0	2
1 mo. " "				.70	.15				0	1
Average	.005	.057	.120	.370	.216	.103	.020	.011	60	7

Table 5a

Numerators show the actual number of times the note was judged as b at various intervals after intensive training; denominators show the actual number of times the note was presented. (The fractions are reduced to decimals for convenience of inspection in Table 5.)

Observer B

	f	g	a	b	c′	d′	e′	f′
Before training:								
Set 1			2/20			4/20	2/20	
" 2			2/20		2/20	2/20	6/20	6/20
3 min. after tr.		1/20	2/20	8/20	5/20			
1 hr. " "			2/20	10/20	6/20	1/20		
1 da. " "			2/20	10/20	5/20		1/20	
1 mo. " "		1/20	2/20	12/20	2/20			

Observer G

Before training:							
Set 1			2/20	2/20			
" 2				10/20	4/20	8/20	2/20
3 min. after tr.			3/20	8/20	6/20	4/20	
1 hr. " "		1/20	5/20	8/20	4/20	1/20	
1 da. " "		1/20	4/20	10/20	5/20	3/20	
1 wk. " "		1/20	3/20	9/20	5/20	2/20	
1 mo. " "			1/20	8/20	2/20		

Observer McG

Before training:							
Set 1		6/20				2/20	
" 2	2/20	6/20	8/20	12/20	14/20	6/20	
3 min. after tr.		3/20	4/20	8/20	10/20	5/20	
1 hr. " "		3/20	6/20	9/20	3/20	3/20	
1 da. " "		3/20	3/20	12/20	7/20	7/20	
1 wk. " "		2/20	5/20	5/20	8/20	2/20	
1 mo. " "				11/20	2/20	6/20	

Observer Z

Before training:						
Set 1	1/20	1/20	8/20	2/20		
" 2		2/20	6/20	7/20	3/20	
3 min. after tr.			1/20	10/20	3/20	
1 hr. " "				12/20	7/20	
1 da. " "			1/20	10/20	6/20	
1 mo. " "				14/20	3/20	

TABLE 38

Numerators show the actual number of times the note was judged as c′ at different stages of learning; denominators show the actual number of times the note was presented. (The fractions are reduced to decimals for convenience of inspection in Table 3.)

Observer A

D. v.		232	240	248	256	264	272	280	288	296
Cents		58.70	56.76	54.45	52.43	50.94	50.19	48.77	47.44	
Test	1	1/10	3/10	2/10	2/10	4/10	5/10	4/10		
"	2			1/10		6/10	7/10	5/10	2/10	1/10
"	3			1/20	6/20	11/20	10/20	5/20	3/20	
"	4				3/20	7/20	10/20	3/20	3/20	

Observer B-C

Test	1		4/10	1/10	3/10	2/10	5/10	4/10	2/10	2/10
"	2			2/10	4/10	0/10	5/10	1/10		
"	3			1/20	1/20	13/20	2/20	4/20		
"	4			3/20	2/20	8/20	7/20	3/20		

Observer F

Test	1	1/10		3/10	3/10	3/10	3/10			
"	2		1/10	1/10	1/10	4/10	2/10	3/10		
"	3			2/20	3/20	11/20	4/20	4/20	1/20	
"	4					12/20	2/10			

Observer H

Test	1				4/10	6/10	8/10	4/10		
"	2			1/10	3/10	7/10	3/10	1/20		
"	3		1/20	2/20	5/20	12/20	4/20	5/20	1/20	
"	4				1/20	17/20	8/20	1/20		

Observer Mi

Test	1			2/20	2/10	4/10	4/10	1/10	1/10	
"	2					5/10		3/10		
"	3					8/10	2/10			
"	4					6/10				

Observer R

Test	1			1/20		13/20	6/20	11/20	2/20	
"	2				1/20	12/20	6/20	7/20	1/20	
"	3					14/20	3/20	6/20	1/20	1/20
"	4					12/20	1/20	5/20		

Baird gives the percentages of octave-errors to correct-note judgments, for 4 of his 9 *Os*, as .31, .22, .10, and .03. Three of our 4 *Os* show corresponding percentages: .27, .21 and .05. Octave-errors are not, apparently, peculiar to the absolute ear.

If, in order to parallel Baird's results as nearly as possible, we consider simply, as he did, the *actual* number of confusions of the different intervals, our order from greatest to least is: seconds, fourths, minor thirds, semitones, fifths, thirds, tritones and sixths, minor sixths, and sevenths. This is not exactly Baird's order, since for him sixths come before fifths; but neither is it an order of community of overtones, and so it parallels Baird's in this respect. Our orders, then, confirm Baird's conclusion that community of overtones cannot be the basis of confusion in judgments of absolute pitch.

The order of frequency for confusion of intervals for our *Os* is not easily determined. If we count, for the moment, octave-errors as *errors*, we have this order:

correct judgments	88
errors of 1 semitone	32
" " " second	55
" " " minor third	37
" " " major third	29
" " " fourth	51
" " " tritone	13
" " " fifth	31
" " " minor sixth	10
" " " major sixth	13
" " " minor seventh	11
" " " major seventh	4
" " " octave	14
" " " " *plus* 1 semitone	6
" " " " " " second	2
" " " " " " minor third	3

When we consider, however, that the *Os* judged in terms of naturals only, and that the number of different intervals possible among the naturals is not uniform, some revision of the above figures seems necessary. On the basis of one diatonic octave there are possible 7 different cases of correct-note judgment, 2 different cases of semitone errors, 4 of seconds, 4 of minor thirds, 5 of thirds, 6 of fourths, 2 of tritones, 6 of fifths, 3 of minor sixths, 4 of sixths, 5 of minor sevenths, 2 of sevenths, 7 of octaves, *etc.* If we divide the number of errors of each interval made by the number of times it is possible for that interval to occur on the basis of the diatonic notes of one octave, the frequency of the errors will be found generally to fall off directly with the size of the interval. See p. 486.

Pre-training Experiments: *Influence of Aufgabe* (*Set 2*). Table 6 shows that under a narrowed *Aufgabe* the *Os* were considerably more adequate, even before training. For, if we select from the first set of pre-training experiments those cases

Proportional number of:

correct judgments	12
errors of 1 semitone	16
" " " second	13
" " " minor third	9
" " " third	9
" " " fourth	8
" " " tritone	6
" " " fifth	5
" " " minor sixth	3
" " " sixth	3
" " " minor seventh	2
" " " seventh	2
" " " octave	2
" " " " *plus* 1 semitone	3
" " " " " " second	5
" " " " " " minor third	75

Baird did not have so difficult a problem in determining which intervals were most frequently confused, since his *S*s did not restrict themselves to judging in terms of naturals, but inspection of his results (62) shows that they rendered more natural than accidental judgments.

where b was presented and judged, we find that it was judged correctly only 5% of the time. The average error, calculated from the notes wrongly judged as "b", is 259 cents. When judgments merely in terms of "b" or "not b" were asked for, they were correct in 36% of the cases, and the error was reduced to 222 cents.

In the case of B, we do not find any improvement under the narrowed *Aufgabe*. Of course if the individual had absolutely no notion of the pitch-region of a note, a heightened degree of attention would be of no avail; and it is true of B that he had no practical experience with musical instruments.

Other casual experiments not recorded here were made by the writer during the summer of 1924 on still other persons, and without exception there was a decided improvement under heightened attention brought about by such narrowing of the task.

Rate of Forgetting. Table 6 shows that this learning persisted for a month, and that it was even slightly augmented by the practice obtained from the test experiments.

Reaction Time. The average reaction time for correct judgments is 5.9 sec.; for incorrect judgments, it is 6 sec. There is even in the individual cases a slight difference in the direction of shorter time for correct judgment, but the difference is never striking.

Direction of Error. As in Group 1, underestimation of the notes presented is commoner than overestimation. (See Table 5.)

TABLE 6

	Average percent. of correct b judgments	Average error (cents)
Before training:		
Set 1.	.05	260
" 2.	.362	223
3 min. after training	.425	94
1 hr. " "	.487	99
1 day " "	.522	94
1 week " "	.450	92
1 month " "	.560	54

Results: Group 3

Procedure. Mu and P undertook to learn all the notes of the chromatic scale from g (196 d. v.) to f# (369 d. v.) inclusive. With P the tonometer was used. Pre-training ability was determined in the usual fashion; intensive training was given by sounding each of the 12 notes 3 times, under attention; and subsequently tests in which each note appeared twice in irregular order were given 3 times a week for 4 months. In these tests auditory distraction was always introduced between notes to prevent the formation of judgments on the basis of interval. After the learning was well under way, P was asked at alternate sessions to make her judgments immediately; in all cases where time was allowed, introspections were called for.

In the summer of 1924, Mu trained herself to the same pitches. No pre-training record could be made in this case because of Mu's experience as *E* in training the *O*s of Group 1. Common brass organ reeds, and the method of random selection at widely separated intervals of time, were found convenient for self-experimentation. Since the reeds were almost uniform in size, tactual clues were inconsiderable in comparison with the primary auditory ones. In this case, all judgments were made *ad libitum*, and except in those cases where the pitch was named immediately, introspections were recorded after each judgment.

Amount of Learning. With respect to the 12 notes to which they were trained, the degree of adequacy attained by both of the *O*s of this group places them easily among those who have absolute pitch. In the case of Mu, the final 96 experiments were 75% correct, and the average error was 27 cents; for P, the final 96 experiments were 62% correct, with an average error of 65 cents. Table 7 shows that again the learning was largely immediate, and in this case the fact is brought out more strikingly than before, since more notes are learned with less intensive training than was the case with either of the other groups. We also find in this group the usual variation in the records from day to day.

Errors. The results of the pre-training experiments of P show most of the characteristics of those of Group 2. Only 5 out of 192 judgments were made in terms of accidentals although accidentals were being presented; "c" judgments were more frequent than those of any other note (38); next in order come the "e" (33) and "f" (31) judgments; "a" stands next with 17. As a group, "c" ,"e", and "f" judgments embrace 9 out of the total number of 13 correct judgments. Errors are chiefly in the direction of underestimation of the notes presented. P, who has had considerable practical musical experience, did not make any octave-errors; her errors never exceeded the interval of the minor seventh.

TABLE 7

	Correct judgments (percent.)	Error (cents) Underestimation	Overestimation
Observer Mu			
Final tests (96 exp's)	.75	15	12
Observer P			
Before training (192 exp's)	.07	333	79
First test after training (24 exp's)	.38	96	54
Final tests (96 exp's)	.62	35	30
Immediate judgments (216 exp's)	.55	53	31
Natural judgments (216 exp's)	.62	25	21

After training, errors are usually less than a minor third. In 455 experiments, Mu never made an error of more than one tone, and in 500 experiments P made only 34. Underestimation was more frequent than overestimation for both *O*s.

Both *O*s made a proportionately larger number of natural than of accidental judgments; but though Mu was slightly more adequate to the naturals, P found the accidentals easier to judge. The individual notes which were best judged included both naturals and accidentals in both cases; for Mu the two easiest notes were g and b, and for P, g and e′.

Immediate vs. Mediate Judgments. Table 7 shows that P's accuracy was slightly diminished when she was asked to judge notes immediately upon their presentation. Baird, on the contrary, found his *O*s somewhat more adequate under these conditions. His *O*s, however, were in this case responding in accordance with their habits, whereas P was accustomed to take her own time in forming her judgments ("natural judgments",

see Table 7); with only a few notes had learning progressed to the stage where immediate response was natural. In the experiments in which she was allowed to take her own time, the average reaction time for correct judgments is slightly less than that for incorrect judgments.

Introspective Results

The objective data show that our *Os* acquired absolute ear for the notes to which they were trained. We have still to consider their introspective reports to discover, if possible, the basis of absolute judgments. First we shall inquire whether there is enough uniformity in the introspection to indicate that there is some single basis upon which absolute judgments rest. The reports do not warrant such a conclusion, for we find a great variety of clues not only among the various *Os* but in the introspections of each individual as well. There are clues from pitch, timbre[54], kinaesthesis, synaesthesia, association, and logical processes, acting sometimes alone, and sometimes in various combinations.

There are, however, a few general statements that can be made. In the first place, all *Os* have recourse to some kinaesthetic placement of the notes, like implicit singing or whistling. In the early experiments these cues frequently served as the principal basis for judgment. More and more, however, kinaesthesis tends to drop out, though it is usually summoned in difficult cases. Concomitantly, there is developed a finer analysis of the timbre of the note, and upon it, then, the greater burden falls. Especially for the 8 d. v. differences do the kinaesthetic cues seem inadequate. This fact is explicitly stated by 4 of the 6 *Os* of Group 1, and is implied in the reports of the other two. A stated that singing did not give a sufficiently exact placement, and that timbre seemed a finer criterion. B-C stated that in these experiments he gave practically no attention to pitch. M said that she determined c′ mostly by timbre, but discarded extreme notes immediately by pitch. (This is the most explicit statement of what is very generally implied in many of the introspective reports. Taken with the facts that extreme notes were in general discarded immediately, it means that pitch is a rough, immediate criterion, which serves to give a rough immediate placement of the note.) R asserted that pitch seemed a rough criterion, and often his reports upon individual cases show that, though pitch seemed satisfactory enough for c′, the timbre was not right for it, and that on the latter basis the note

[54]By "timbre" is meant gross qualitative differences between notes that are not fundamentally differences of pitches. The physical nature of these qualitative differences is unknown.

was rejected. It is also apparent from the reports of F that he usually relied upon a feeling of familiarity based upon pitch and timbre. H definitely passed from the stage where kinaesthesis predominated to that where he says he found relaxation better, with attention to timbre. The synaesthetic, associative, and logical cues are comparatively few, and were used by only 3 *O*s, chiefly in the early experiments. They include such experiences as colors, visual placements, sounds of boat whistles and sawmills, and such reasoning as that a note which came early in one series would come late in the next.

In general, also, the *O*s pass from a rich experience in the early experiments to a poorer one, which is sometimes so immediate that it cannot be analyzed at all. Sometimes the basis of judgment can be described as a feeling of familiarity, and sometimes as an outstanding feature of the total *Gestalt* of the note.

With respect to the correlation between speed and accuracy of judgment, we have already noted that in Group 2 there is too slight a correlation to be statistically valid, but in this group training did not proceed very far. With P, forced immediate judgments were found to be somewhat less accurate than the slower natural ones; but where the judgments were made *ad libitum*, reaction time for right cases was found to be slightly less than for wrong ones. It seems probable that with further training the degree to which our *O*s would make immediate judgments would approach that shown by Baird's.

Quantitative treatment of the introspection of P and Mu shows that the majority of their judgments were made chiefly upon the auditory clues offered by the note. It is impossible to say to what extent obscure kinaesthesis may have been present and determinative. Nevertheless, auditory features, especially of timbre, furnished the outstanding clues. Rarely did it happen, however, that pitch was so much overlooked that a very different pitch of similar timbre was mistaken for the correct note: the two criteria of pitch and timbre were not so divorced. Rather does it seem that, after a very rough initial placement of the note as high, low, or intermediate, some feature in its relation to the total tonal configuration furnished the basis for judgment. In difficult cases, kinaesthesis was consciously introduced, more often successfully than otherwise. Sometimes decision was arrived at by a process of the mental elimination of neighboring notes.

To the *O*s of all three groups, outside disturbances were very distracting, and requests for repetition of the presentation of notes were frequently made.

Summary of Results

(1) Average persons frequently possess absolute ear in some degree inasmuch as they can judge notes correctly, previous to training, in a greater number of cases than would be due to chance.

(2) Ability to judge notes correctly can be greatly improved by training, and in the region of 264 d. v. can be extended at least to notes only 8 d. v. (about 50 cents) apart.

(3) The improvement is largely immediate, and is relatively lasting in its effects.

(4) In making judgments of absolute pitch a high degree of attention to the notes is more effective than a lower degree; and a note which is not attended to has no noticeable effect.

(5) The majority of errors are small, and the frequency of error falls off directly as the size of the interval increases. Previous to training, some octave-errors occur.

(6) Errors of underestimation predominate for both men and women in the region between g (196 d. v.) and f′# (369 d. v.)

(7) A high degree of transfer of learning occurs when various organ-stops are used.

(8) There is a very slight correlation between speed and accuracy of judgment, except where the *O* is forced, prematurely, to make all his judgments immediately; then accuracy is slightly diminished.

(9) Ability to acquire absolute pitch correlates better with practical musical ability than with ability in the Seashore tests.

(10) From the introspective reports it appears that, in the course of learning, experience becomes less rich. Kinaesthesis, especially of the throat and mouth, has an important part in the beginning, but tends to give way to auditory features of the note. When judgment is difficult, kinaesthesis is summoned as an aid, and it is possible that kinaesthetic factors are always obscurely, if not clearly, functioning.

In the later stages of learning there is, typically, a rough immediate placement of the note presented for judgment; and, secondly, a finer placement made upon a clue furnished by some out-standing feature of the total tonal configuration. Sometimes judgment is based simply on a feeling of familiarity, and sometimes it is too immediate for analysis.

(11) There is no objective evidence which necessitates the postulating of an attribute of quality, or tonality, which recurs in successive octaves, and upon which all judgments of absolute pitch rest; nor do the introspective reports disclose the existence of any such attribute.

In connection with these results upon the specific question of the acquisition and nature of absolute pitch, there have been some which have a wider bearing; namely, those which show the influence of *Aufgabe*. It was found that when, previous to any training, *O*s were asked to name every note presented by its own name, they were less adequate to any single note than they were when asked to judge all the notes simply by reference to the name of one particular note. It was also found that, where the task was harder, as in the experiments with 8 d. v. differences, the frequency of correct judgments was less, but the number of large errors was also less, than in the easier experiments where only diatonic notes were presented.

Conclusions

(1) *The average person can acquire absolute pitch.* This statement is based on the fact that there is a similarity of performance between Baird's "gifted" *O*s and our group of average persons after intensive training. Similarity between the two groups is shown in respect of degree of accuracy, of nature of errors (size, direction, and individual differences), and to some extent in respect of immediateness of judgment, inasmuch as judgments became more immediate as learning progressed and, even where immediate judgments were forced, a high degree of adequacy was maintained. Indeed, we may go further and say that it seems true that most persons who are familiar with music have absolute ear in some degree even previous to training.

(2) *The possession of absolute ear seems to rest simply upon the giving of attention to notes as phenomena*: that is to say, upon an interest in the notes themselves rather than in their melodic or harmonic relationships, which, because of their much greater musical importance, usually monopolize attention. The importance of attention is indicated (*a*) by the immediateness and permanence of the learning; (*b*) by the individual variability from day to day; (*c*) by the increased accuracy under a narrowed *Aufgabe*; (*d*) by the fact that a stimulus which is not attended to has no noticeable effect; and (*e*) by the fact that a good hearing of the note is necessary before judgment can be made.

There still remains the question why certain individuals, without having been trained, possess absolute ear so much more completely than others. An answer may lie in the suggestion that such persons, probably when they were children, found notes interesting in themselves, perhaps because of an unusual degree of ability to deal with notes in this way. Vivid associa-

tions which the notes suggested,[55] establishing them thus in their own right and then in time fading out, may have been the counterpart of the experiences which our *O*s underwent in learning. In support of this suggestion it may be said that many persons who have absolute ear discovered it when they were very young; and that, in certain schools of music which claim to teach absolute pitch, frequent successes are said to occur with the children, who are taught according to the system of "fixed 'do' ", in which each note has only one name,—"c" = "do", "d" = "re", etc.,—acquiring thereby more individuality than in the system of "movable 'do' " commonly taught in the public schools.

---

[55]R. Rolland, *Jean Christophe*, I; F. Galton, *Inquiries into Human Faculty*, 1883, 99, 182, 183.

THE PSYCHOLOGY OF *GESTALT*[1]

By HARRY HELSON, Cornell University

TABLE OF CONTENTS

		PAGE
(10)	General Considerations	494
(11)	The Figure-Ground Configurations	495
(12)	The Perception of Movement	498
(13)	Configurational Physiological Theories of Movement	514
(14)	Formal Spatial Effects Due to Configurational Properties	519
	(*a*) Stereoscopic Vision	519
	(*b*) Depth, Rivalry, and Related Phenomena	520
	(*c*) The Perception of Two Colors, the One Behind the Other	522
(15)	Configurational Theories of Space Perception	524

(10) *General Considerations*

In a previous discussion of the theoretical aspects of configurationism I endeavored to show how the members of the school found their psychology anew upon a set of assumptions which shall guarantee the integrity of phenomenal and behavior patterns. The configurationists not only deny that the formal properties of perception and action can be derived from a knowledge of elementary sensational units or part responses like reflexes, but they also assert, in establishing the constructive part of their programme, that the form or structure of a complex, integrated whole exercises a constitutive effect upon the elements within the structure, and changes them in a manner predictable only from a knowledge of configurational laws.

It will be remembered that the main object of the configurational psychologists is to account for a given phenomenon without destroying it by a direct, frontal analysis. In place, then, of the older introspective or behavioristic analyses of perception and behavior, the configurationists substitute functional situations in which a change in conditions will result in a change in the fact studied. Functional variations are the chief means of formulating configurational laws. This method of procedure is in line with the demand that descriptions and explanations of psychological data must apply to the *phenomenal reality*, whatever it may be. Instead, therefore, of asking *what a given fact is*, the configurationists ask "*What is the function which determines the phenomenon?*" It is this point of view which has

[1]This article is the second of a series on the psychology of *Gestalt*, the first of which appeared in this JOURNAL, 36, 1925, 342-370.

led them to test old assumptions and to formulate new hypotheses in an endeavor to bring larger and larger classes of facts within the sphere of experimental psychology.

One can easily see that the configurationists have little difficulty with the stimulus-error or the terms to be used in *Beschreibung*. Since the observer is not called upon to analyze his experiences, but rather to report the object of his perception, no special psychological attitude or training is necessary. Hence we find untrained psychologists, men specializing in other subjects, and experts, taking part in the same experiments and agreeing very well in their reports. If the conditions of the experiment have been set and the observer is careful and of normal intelligence, even though he is psychologically näive, the problem of description is no different in psychology from what it is in the other sciences,—since the same (configurational) principles obtain in all the objective disciplines. To be sure, many facts require practice and training in observation, as the configurationists themselves admit; but this requirement is no peculiarity of psychology alone, and may well be expected in a science dealing with complex integrations like perceptual and behavior patterns.

Most of the facts studied by the configurationists require a totalizing or synthetic attitude favorable to the perception of, and the reaction to, wholes. Many observers seem to fall naturally into what has been called the analytic attitude, and often fail to report on configurational phenomena or do so only after practice and with difficulty. We shall have occasion to note the differences between those who do and those who do not react readily to configurational situations.

In this paper I shall trace the development of configurational theories by a consideration of the experimental findings upon which they are based.

(11) *The Figure-Ground Configurations*

The phenomenological study of figure and ground made by Rubin[2] appears to fit in so well with many of the conclusions reached independently by the configurationists that Koffka and Köhler have quoted this writer's work in detail as confirmation of their own main contentions, *viz.*, that there are no isolated sensations in experience, that analysis destroys phenomenal properties of perception, and that the facts of memory, recognition and behavior require the configurational point of view if they are to be made amenable to any general theory which shall embrace them all. In place, therefore, of descriptions of experience in terms of elements, Rubin and the configurationists would describe it as made up of definite configurations in which a figure protrudes from a more or less undifferentiated background. Figure and ground represent two fields, the

[2]E. Rubin, *Visuell wahrgenommene Figuren*, I, 1921, VII-244.

one of which predominates while the other sets it off. Qualities emerge from a general level, and hence must be considered with reference to their background if their properties are to be fully accounted for in perception. The presence of figure and ground in perception is necessary for the emergence of any qualities whatever; otherwise experience would present us with undifferentiated fields to which reactions would be impossible. Moreover, the fact that one is always conscious of, or reacting to, at least two fields in mutual dependence precludes one from describing experience by means of unit-elements or constant, independent factors like sensations, reflexes, or fixed associational mechanisms.

The distinction between figure and ground is avowedly phenomenological. It we examine our perceptions as we find them, we observe that certain parts of the perception have a clear, well-defined pattern while the rest is more or less homogeneous and in the background. One field may be called the figure, or positive field, while the other is the ground, or the negative field. It is often possible, with ambiguous figures like the reversible staircase, to change figure into ground and conversely. The usual explanation of this phenomenon, as Koffka points out,[3] is that attention turns toward different parts of the figure and makes them successively more vivid than they otherwise would be. The difference between figure and ground would then be merely a matter of difference in clearness. But, for Rubin and the configurationists, figure and ground possess different phenomenal properties, and are therefore two wholly different objects when perceived now as figure and now as ground. The object-character of any field depends upon its being the figure, having a definite form and structure. A thing is stuff *plus* form, ground become figure. The ground, however, must not be regarded as wholly unformed. The ground possesses certain vague, positive determinations; for one can say of it that it is a broad, extended surface, or a small rim around the figure.

The contention of the configurationists that phenomenal differences represent real differences in configurations is based upon the fact that a field when seen as figure possesses a higher limen for color induction than when seen as ground.[4] Katz' distinction between surface and film colors holds for figure and ground.[5]

---

[3] K. Koffka, Perception; An Introduction to the *Gestalt-Theorie*, *Psychol. Bull.*, 19, 1922, 531-581.

[4] A. Gelb and R. Granit, Die Bedeutung von "Figur und Grund" für die Farbenschwelle, *Z. f. Psych.*, 93, 1923, 83-118.

[5] D. Katz, *Die Erscheinungsweisen der Farben, etc.*, *Z. f. Psych.*, Ergänz. Bd. 7, 1911. *Cf.* M. F. Martin, Film, Surface and Bulky Colors and their Intermediates, this JOURNAL, 33, 1922, 451ff.

The film colors are not localized definitely, lack form, and are soft and yielding, belonging to the ground; the surface colors, which are more stable, seem to belong to the figure.

Experiments in which ambiguous[6] fields were presented to *Os* showed that, once a field has been perceived as figure, "an after-effect exists which expresses itself in a tendency to experience in the succeeding trials the same field as figure which was seen as figure the first time."[7] This is called the figural after-effect, and according to Rubin cannot be explained by association. Association implies, he says, a connection between at least two elements, whereas in his experiments the same figure was recognized in succeeding trials without the correlative associative factor; only one figure was seen each time. There is a process at work analogous to association, we are told, but association does not adequately cover the fact that everything related to the figure is remembered better and centralizes better than what is related to the ground. The figure is more impressive than its ground, and may be connected with other figures more easily than connections can be formed between one ground and another.

The contour runs between figure and ground and is the common boundary of the two fields. Witasek[8] regards the contour as in itself a perceived configuration; but Rubin asserts that only the surface forms a configuration. The contour affects the figure more than the ground in view of the fact that it determines very directly the figure's form. If the contour were not present to differentiate the two fields, no figure would be present, and no meaning or significance would attach to the presentation. Contours thus affect the whole visible world as it appears to us.

For a thoroughgoing phenomenological point of view one should read Rubin's account of such perceptual complexes as the surface-figure, contours, lines, pointed properties, angular relations, breadth and distance. In this discussion he emphasizes differences which experience presents to everyday perception. I give two examples as illustrative of a method which substitutes phenomenological descriptions in the place of analytical descriptions. An angle, we are told, is the result of the relations between two contours, not the property of either one of them. The surface included by the contours is a property of the totally experienced figure and may be called a prong (*Zacke*). Prongs may be pointed, or blunt, or neither. For most people a prong does not seem pointed if it has an angle of 90°. Or consider the phenomenological differences between a surface and a contour: a surface has no parts, being all of a piece, while the contour which has parts constitutes a unity. If an *O* is asked to describe a square piece of paper, he will picture it as a formed surface without parts, while the contour will be described as a unity arising from parts. Phenomenological differences of this sort may be of great importance in determining the actual constitution of one's perceptual world and the reactions one makes to it.

Koffka has applied some of Rubin's phenomenological descriptions to the facts of child psychology, and derives the concept of the configuration from the more general fact of figure and ground as follows. "The first phenomena are *qualities upon a ground*. They are the simplest mental configurations. The phenomenal appearance in consciousness divides it-

[6]Not all figure-ground configurations can be reversed. Some fields can not be changed from figure to ground and conversely. Really, Rubin deals only with what Werner has called "poly-figurations" or configurations capable of a number of phenomenological phases. "Mono-figurations", in Werner's terminology, cannot be reversed. H. Werner, Rhythmik, eine mehrwertige Gestaltverkettung, *Z. f. Psych.*, 82, 1919, 189-218.

[7]Rubin, *op. cit.*, 13.

[8]S. Witasek, *Psychologie der Raumwahrnehmung des Auges*, 1910, 348ff.

self into a given quality and a ground on which the quality appears... It is, however, a part of the nature of a quality that it should lie upon a ground.... Such a coexistence of phenomena in which every member 'carries every other,' and in which every member possesses its peculiarity only by virtue of, and in connection with, all others, we shall henceforth call a *configuration*. According to this view the most primitive phenomena are figural."[9] Order, according to the configurationists, is therefore present from the very beginning, since the perceptual and reaction consciousnesses arise only when an emergent from a general level greets the new-born individual.

The concept of figure and ground provides the configurationists with a means of investigating functional differences within a given situation. Its phenomenological value is evident, since it can apply to any sense modality and to behavioristic as well as to phenomenal data. This may not seem possible at first glance; but a sufficiently broad use of words enables one to make the application. Thus we may say that sounds arise from a background of other sounds or stillness; tactile experiences may emerge from diffuse surface or organic pressures, pains or the like; in short, any experience appears to the percipient as either a continuation of some previous one, on the same level, or as a rise or fall from a level, or as a change in figure within a given level. From the point of view of the behaviorist, the first reactions are not to "simple" stimuli, but rather to situations which present "step-wise" or differential aspects fundamentally related to figure and ground. Figure and ground may thus be said to be the simplest configurations which are observable in perception and to which a discriminatory response can be made.

(12) *The Perception of Movement*

It is within the field of the perception of movement that the configurational theories took their rise and have received their most systematic application and development. It will therefore be necessary to examine in some detail the experimental work bearing on the problems of apparent movement, if we are to understand why the configurationists have advanced their theories, on what evidence they base them, and in what direction configurational methods are tending. Our survey of Rubin's work on figure and ground has shown to what lengths phenomenological descriptions may go, but it has not brought out what the configurational work on movement will show, *viz.*, the effect of varying specific conditions within a total complex in such wise that laws can be stated in terms of the variables responsible for the phenomenal pattern; in other words, how functional dependencies can be traced in configurational phenomena.

The reason that the field of apparent movement has been so productive of results is, perhaps, that it offers some of the most

[9] K. Koffka, *The Growth of the Mind*, 1925, 131.

difficult paradoxes to be met with in experience, and hence requires a theory which shall embrace a large number of facts which at first sight do not seem to harmonize into any single scheme. The most noteworthy attempt of the configurationists has been to explain the facts of real movement, apparent movement, and phase movement, whether within a single sense modality or across sense modalities (movement from touch to vision!), by means of one set of assumptions and principles.

Let us first note the various possibilities under which movement may be perceived. There is, of course, the perception of a real, single object in motion. The problem of inferred motion, like that of the movement of the hour-hand of a watch, does not here come into consideration. If two stationary objects are exposed in quick succession, as in the stroboscope, they may be perceived as a *single* object in motion. Even a single stationary object may, under the right conditions, be seen as moving. A single moving object may be seen as at rest in different places at different moments. Finally, a number of cross-sectional stationary phases of moving objects, as in the moving picture, may result in the appearance of movement. Any theory of movement must account for real movement, for the apparent movement of stationary objects, for the failure to perceive real movement when it is present, and for the perception of movement when successive phases of a moving object are seen. From the phenomenological point of view apparent movement may possess all the descriptive properties of real movement; hence to explain away apparent movement by designating it as illusory does not account for the fact that real and apparent movement appear so much alike they often cannot be differentiated.[10]

From our discussion of the divers ways in which movement may be perceived or may fail to be perceived it is clear that the phrase "the perception of movement" is highly ambiguous. It becomes even more ambiguous when we inquire into the meaning of the word "perception"; for as soon as we do this we become involved in the problem of the psychologically real. If we consider perception as the end-result of a stimulus, then stroboscopic movement is not real, since nothing real has moved; and we have to explain the 'illusion' of movement. But in view of the fact that *O*s are unable to distinguish between real and apparent movement on the basis of their immediate experience, apparent and real movement seem to possess the same phenomenal properties; so far as psychology is concerned, we have no reason for calling the one illusory and the other real. It must be granted that without perception there would be no move-

[10]F. L. Dimmick and G. H. Scahill find descriptive differences between real and phenomenal movement. *Cf.* Visual Perception of Movement, this JOURNAL, 36, 1925, 412ff.

ment of any kind, since movement depends upon the apprehension of a stimulus during at least two instants of time and through two points of space. Hence, as Kehr has pointed out,[11] the perception of movement is a special case of perception in general, and may throw light upon the whole problem of perception.

A considerable amount of experimentation preceded Wertheimer's investigation of apparent movement, and out of this experimentation a number of theories had been advanced to explain the observed facts.[12] All the theories were built upon the assumption that the perception of movement could be analyzed into simpler constituents, or at least might be correlated with physiological processes and the sensations which accompany them. Wertheimer's paper of 1912 contains the results of a series of experiments designed to test the prevailing theories and the assumptions underlying them. Some idea of the confusion in the field may be gained from the different views of movement held by various writers as a result of their analyses: according to Marbe apparent movement is due to a fusion of after-images; Wundt also held a peripheral explanation in terms of eye-movement; Exner, Stern and others maintained that movement requires a special kind of sensory process, thus adding to the growing list of sensory elements; still another theory, to mention only one more of those in the field, implied that the perception of movement required a founding process, analogous to the founding process of Ehrenfels by which the form-quality is produced over and above the founded sensory elements. In all these theories the assumption is made that perception can be broken up into something simpler which will correlate directly with the stimuli. None is concerned with the necessary and sufficient conditions of all kinds of perceived movement and with the endeavor by their means to connect it with other phenomena which are admittedly simpler and more fundamental in origin. The upshot of the experiments conducted by Wertheimer may now be briefly stated: the perception of movement is a perception *per se*, not a perception involving new elements, but rather a perception differing from other perceptions in certain describable attributes which are lost in the process of direct analysis. Having found adequate descriptive terms for the phenomenon, Wertheimer proceeded to correlate changes in the objective conditions with changes in the *experience of movement*,

[11]Th. Kehr, Allgemeines zur Theorie der Perzeption der Bewegung, *Arch. f. d. ges. Psych.*, 34, 1915, 106ff.

[12]M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Z. f. Psych.*, 61, 1912, 161-278. A bibliography of the literature and theories mentioned below is given in this article. The argument and the facts from here onward follow this article very closely.

making a functional or indirect analysis of the facts he was studying.

Wertheimer's method of procedure was, in general, as follows. Two black lines were exposed in succession on a white background, the one vertical, the other horizontal, in such fashion that if both were seen simultaneously they would form a right angle. The colors of the lines and of the background were varied. Figures of various forms and in various arrangements were introduced and observed. With slow rotation of the stroboscope first the one line appears at rest and then the other. Quicker rotation causes both lines to appear stationary at the same time. If the speed is now adjusted between these two speeds, the one line appears to move over to the other, and often returns to its original position. This is Wertheimer's optimal movement or "phi phenomenon." Other kinds of movement may also be seen under conditions which are now known: each line may move by itself, independently of the other, and this Wertheimer calls successive movement or dual part movement; the stage in which both lines are seen at rest is called the simultaneous stage.

A number of questions arise concerning the part which the time interval between the stimuli plays in determining the kind of movement seen and the influence of other variables, notably the order of presentation of the stimuli, the position, mutual distance, form and color of the objects seen in movement. Subjective factors like fixation and attention were studied and controlled. Later a third object was introduced into the field of vision, to complicate the situation, and its effect upon the movement was noted. The influence of after-images and other after-effects was also taken into account.

We may summarize Wertheimer's results briefly as follows. Optimal movement occurred with most *O*s when there was a pause between the stimuli of 60 σ. Stationary simultaneity appeared at 30 σ, while stationary succession of each stimulus occurred at about 200 σ. A more detailed discussion of the influence on movement of the factors mentioned above will be given in the consideration of later work. Let us consider the facts advanced by Wertheimer against the prevailing theories of movement.

The evidence against the eye-movement theories seems conclusive, since the time required for movement (60-100σ) was less than the minimum required for eye-movements (130 σ). Furthermore, fixation was so controlled as to reduce or rule out eye-movements; and when after-images were projected upon a fixed spot they remained stationary while the lines moved. Finally, several movements could be seen simultaneously in different directions, a fact which precludes the possibility of the eye-movement explanation for these cases.

Theories involving a union of the lines stroboscopically exposed, whether by means of founding central processes or of peripheral fusion processes, are also rejected by Wertheimer. So, too, the central completion theories are discarded, because the optimal conditions show a single, identical thing moving, which may change in color or length while in motion. The fact that a single moving object is seen is not to be confused with a conviction or memory or imagination that one object moves; it is not a matter of seeing one line and then another and then adding movement to them. In fact, *O*s cannot distinguish between this apparent movement and that of a single moving object. Of course, various kinds of movement may be seen under varying conditions by one and the same *O*; for example, continuous, jerky, single, and part or dual movement were all reported.

Some further observations have interesting theoretical consequences. The introduction of a third object into the field of movement, either like the other two or differently colored and formed, may change the original movement. With proper fixation the third object appears at rest while the other two move; or the third object may move independently of the original two. Two parallel lines, *a* and *b*, may be exposed with two lines *c* and *d*, forming an angle, *a* opposite *c* and *b* opposite *d*. Exposures favorable to movement of the lines forming the angle, *c* to *d*, were not favorable to movement of *a* to *b*. But if one of the parallels, *a*, is exposed with the oblique line *c*, then good movement appears of *a* to *b* as well as movement from *c* to *d*. And this movement would take place when the exposure of *a* and *b* alone gave almost no movement. Wertheimer compares this result to a dead interval which is quickened into life by a live one in its vicinity. It may be thought that since *a* was connected with *c*, and *b* with *d*, therefore good movement took place from *a* to *b*; but since good movement took place from *a* to *b* when *c* and *d* were unnoticed, this explanation does not hold. Nor do the lines *c* and *d* partially absorb the attention, because a lack of attention is unfavorable to the perception of movement. Experiments with distraction from *a* and *b* did not give a better movement. The effects of attention receive detailed consideration in the paper we are discussing and in the papers on movement which are to follow; but it may be stated in general that the configurationists do not believe that the explanation of apparent movement is to be found in processes going on at the periphery of attention; in fact, the best movements occur at the center of attention or are enhanced by "concentration of attention."

The unitary, unanalyzable nature of the phi phenomenon, which the configurationists have stressed, centers largely upon the following facts: (1) for the most part nothing was seen of the lines; "something moves" is all that can be said; (2) only one of the lines may appear; (3) parts or pieces may appear, but such parts or pieces may not be seen until after the completion of the movement. Some *Os* even reported that they saw movement but no object which moved; this pure movement Wertheimer called "pure phi"; it is incapable of further analysis or of any description other than that 'movement was seen'. Wertheimer concludes that movement is not built on stationary elements and cannot be analyzed into static elements; phenomenally, the perception of phi possesses all the descriptive attributes of real movement. To call it an illusion of judgment would be false, because full knowledge does not alter the seen movement. Nor does the central-completion theory suffice, because the movement may occur with no semblance of an exciting stimulus. With red lines on a black ground, the one line moved over to the other but no red appeared, while an absence of color was noted when white lines on a black field were exposed. Pure phi contains no perception of line, color, content or founding process. Wertheimer's characterization of pure phi phenomena is as follows: "Im Gegensatze zu anderen psychischen Gegebenheiten sind sie nicht statischer sondern dynamischer Natur; in dem spezifisch charakterisierten 'Hinüber' usw. haben sie ihr psychologisches Fleisch und Blut, nicht zusammensetzbar aus dem der üblichen optischen Inhalte."[13]

Whether or not such a thing as pure phi exists is after all a secondary matter compared with the main result of Wertheimer's experiments. The facts which he discovered led Wertheimer to the conclusion that perception cannot be analyzed into simple, sensational units and explained by their means. An explanation of apparent movement, or of any other perception, must spring from an entirely different source,

[13] *Ibid.*, 227.

namely, from a functional variation of conditions which are responsible for, favor, or hinder it. Only in this way can laws of perception be formulated and the facts of one domain be related to those of another. Any theory of the perception of movement must therefore embody the facts of movement *qua* movement and must embrace the physiological antecedents of the psychological event. The configurational theory proposed by Wertheimer was designed to fulfill these conditions and to stimulate further investigation. The theory must explain optimal, dual and successive movements; it must explain pure phi,[14] colored movement, and the simultaneous stage; in short, any theory must account for all the facts of movement as distinguished from all the other varieties of perception we know.

The configurational principle advanced to explain the perception of movement has not only been extended to include all the facts of perception but it has also been erected into an universal postulate on which the configurational psychology has been reared. The psychological and physiological processes involved in the perception of movement must be regarded as total, structured processes, unified in time. The perception of movement is a configurational phenomenon because it represents an unified experience which cannot be analyzed into simpler elements without forfeiting the peculiar *quale* by which we recognize the experience; furthermore, in the experience of movement there are super-summative properties not derivable from the properties of the parts as such, And the configurationists maintain that these statements hold true of all perception, memory, thinking, acting,—of experience in general. The investigation of configurational phenomena is not limited to psychology. Psychology, which depends upon physics, chemistry, biology and related sciences, requires concepts which receive confirmation in these other fields. In this way natural-scientific laws can be formulated in psychology, enabling psychologists to explain, control, and predict the data which they observe. Let us see how configurational principles have been elaborated along these lines.

Since the phenomena described by Wertheimer are in some sense 'illusory', arising from the successive exposure of two stationary stimuli, the

[14]Although Koffka and Köhler are inclined to accept the fact of a pure phi, objections to it have been raised. Benussi repeated Wertheimer's experiments both in touch and vision with 27 *Os*, and found that "something moves" even though the *Os* could not tell what moved. Benussi says that "experience gives no pure movement in the sense of a movement without an object." V. Benussi, Versuche zur Analyse taktil erweckter Scheinbewegungen, *etc.*, *Arch. f. d. ges. Psych.*, 36, 1917, 65ff. Dimmick's *Os* reported that the movement could be analysed into a "gray flash in time." *Cf.* F. L. Dimmick, An Experimental Study of Visual Movement and the Phi Phenomenon, this JOURNAL, 31, 1920, 317-332, and the later article referred to in Note 10.

question arises concerning the effect of figures of the Müller-Lyer type upon the kind of movement perceived. In other words, if two figures are exposed in succession, the one of which *seems* larger or smaller than the other, will there be a difference in the kind of movement seen as compared with that produced by two stimuli which appear to be equal in size? Will the figures seem to grow or shrink in size as the second one is perceived? This is equivalent to asking whether the phenomenal configuration can have any effect upon the kind of movement perceived, whether the figural factor in a situation is in any sense a really causal factor. Other problems also call for solution: the effect of changing the distance and the time between the stimuli; analytic *versus* synthetic attitudes on the part of the *O*; the direction of attention and the rôle of fixation; the effect of the second stimulus upon the first. These and a number of other questions were answered by Kenkel, who used the Schumann tachistoscope in which two figures were exposed in quick succession and were observed either in one exposure or in a number of serial exposures.

Three kinds of movement are described by Kenkel, according as they arise from figures of the Müller-Lyer (or illusory) type, denote the real differences in the size of the stimuli, or concern only one stimulus. The three kinds of movement are differently labelled. *Alpha* movement depends upon illusions of magnitude. In *alpha* movement the parts of figures exposed successively seem to change in size, growing larger or smaller, as they move; *e. g.*, the central line of the Müller-Lyer figures. *Beta* movement depends upon actual differences in the size of the stimuli and involves a change in the magnitude of the figures as the first moves into or grows into the second. *Gamma* movement concerns the movement within a single figure as it expands and contracts when it is exposed and as it is fading from view. Descriptively, the *alpha* and *beta* movements are qualitatively like each other; so that we must admit that the *apparent* magnitude of a figure exercises the same effect upon the perception of movement as do *real* differences in magnitude.

"The *alpha* movement," we are told, "can rest only on one fact: the configurational process released by the second figure is so modified by the preceding configurational process that between the two processes phi-*alpha* arises alongside of the *beta* movement conditioned by the different spatial positions of the two stimuli."[15] In other words, both stimuli must be regarded as forming a single configurational whole. Memory, past experience and the like cannot be used to explain the facts, although practice was often necessary in order to see the phenomena at their best. Often it was possible to see all three movements within the compass of a single exposure.

From the configurational point of view the experiments of Wertheimer and Kenkel are important because they show that the various factors in the stimulus-situation must be taken as a whole, as well as the perception itself, if phenomenal movement is to be seen. Moreover, the figure or form of the stimuli, their arrangement with regard to each other, and their actual spatial patterns, are all determinants of the perception. The term configuration (*Gestalt*) includes not only the spatial configuration but also any and all factors which may change the experience, such as the time between the stimuli, the spatial distance, the intensity, and whatever else affects the movement. In fact, we may say that a configurational system is a complex

[15] F. Kenkel, Untersuchungen über den Zusammenhang zwischen Erscheinungsgrösse, *etc.*, in *Beiträge zur Psychologie der Gestalt*, edited by K. Koffka, 1919, 49.

in which a change of any one part results in a change in the whole. If the total complex is intricate and involved, then it takes a longer time to comprehend all the parts within a unitary figure, and longer exposures are necessary if good movement is to take place. An analytic attitude destroys movement, as practice helps it to appear. Benussi and Wertheimer had shown that exercise in a given kind of movement makes its reappearance easier, so that even in configurational phenomena the effects of past experience come into play.

But other factors may determine the perception of movement, factors which, unless we call the total complex of subjective and objective conditions a configuration, would seem to lie outside the configuration proper. Attention, if it is not constant, may upset the whole phenomenon, changing the apparent movement both quantitatively and qualitatively. Both Benussi and Schumann had noted this fact earlier.[16] Well-divided attention over the whole field favors the appearance of the various kinds of movement. Moreover, the "constitution" of the *O*, his attitude, his previous practice and past experience, may play a part in determining the kind of movement seen. All of which goes to show that subjective factors apart from the objective configuration may be of tremendous importance in determining what will be perceived and in making against the constancy of configurational phenomena. To take care of all the variables within the organism, as well as of those without, in a single causally-coherent configurational system, Kenkel gives expression in the following way to what may be called the total configurational structure in which phenomenal movement is enmeshed: a configuration appears in experience in a certain form and size determined by the physical stimulus, the retinal image, and the complex K. The physical stimulus is constant (or may be kept so), while the complex K in the organism is not constant, depending upon the direction, division and strength of attention, the configurational dispositions, attitude, practice, and a number of other variables at present unknown. It would therefore appear that the configurationists can hardly dispense with the older concepts of attention, attitude, practice and the like, to the extent to which their initial programme committed them. So many variables must be included within the configuration that, in its broadest sense, it means no more—and no less—than the totality of conditions both outside and inside the organism.

[16] V. Benussi, *op. cit.*; F. Schumann, Beiträge zur Analyse der Gesichtswahrnehmung, *Z. f. Psych.*, 30, 1902, 289. Benussi asserts that attention is a necessary condition of the appearance of illusions, just as it is of many other phenomena of perception, but it is not a sufficient condition. *Cf.* also an article by Benussi, Stroboskopische Scheinbewegungen und geometrisch-optische Gestalttäuschungen, *Arch. f. d. ges. Psych.*, 1912, 24, 31ff.

Whether the factors within the organism can be reduced to processes amenable to the laws of physics and chemistry or included under the rubric of sensational experiences has been a matter of controversy between the new configurationists and the Graz configurationists. Benussi had investigated the types of movement described by Kenkel, and treats them as illusions subject to the laws of illusion or as "supra-sensuously" conditioned phenomena.[17] According to Benussi's theory, if an illusion is to make itself perceived, a synthetizing attitude on the part of the *O* is necessary. Exercise in the synthetic attitude is accompanied by fatigue in the analytic attitude and conversely (law of double exercise). There is thus an exercise-fatigue equivalence. Illusions arise from an "inadequacy process" conditioned by the inadequacy carriers in illusory figures: in the Müller-Lyer figures the inadequacy carriers or factors favoring the illusion are the arrow-heads and feathers, in the Zöllner figure they are the transverse lines. So any factor favoring the illusion will aid the *alpha* movement. Benussi found that with the extreme analytic attitude the *alpha* movement could be made to disappear entirely. Kenkel does not mention this fact.[18]

The difference between Koffka-Kenkel and Benussi is regarded by the latter as merely a difference in terminology; but it is more than that, since Benussi's *Produktionsvorgang* (the special psychical act responsible for the inadequacy process) determines his whole theory of perception, and results in a bipartite psychology of sensation and perception, of subjective and objective processes.[19] The new configurationists insist that all the facts must be explained by means of a single principle accounting for discrepancies between sensational and configurational experiences and the stimuli from which they arise. There can be no doubt that Benussi has emphasized an important point with regard to configurational experiences, namely, their variability as opposed to the apparent constancy of the sensational or qualitative aspects of perception. Just as the configurationists have stressed the lack of agreement between sensation and stimulus, so Benussi has insisted that configurational phenomena are subject to distortion as compared with the objective situation. The fact that configurational perceptions are no more constant than sensational processes, and the resulting discovery of the analytic and synthetic attitudes as important determinants of configurational perceptions, have opened up a wide field for investigation concerning the perception of wholes and complexes.

Further investigation of the effect of the objective configuration and subjective factors upon the *gamma* movement was made by Lindemann, who exposed a single object tachistoscopically under various conditions.[20] Lindemann studied the effect on *gamma* movement of varying the exposure time, the intensity and size of the objective configuration, and subjective factors like attention, fixation and fatigue. The results were confirmatory of Kenkel's findings; but *gamma* movement is not a result of attention alone. Rather, Lindemann's general conclusion is stated in terms of con-

[17]V. Benussi, Gesetze der inadäquaten Gestaltauffaussung, *Arch. f. d. ges. Psych.*, 32, 1914, 396-419.

[18]It will be remembered that Schumann, in his four articles on the illusions, emphasized the fact that the method of "total impression" favored the appearance of the illusions. More will be said later concerning the analytic and synthetic types of observation and their effect upon configurational phenomena.

[19]Benussi, *opp. citt.*, and Experimentelles über Vorstellungsinadäquatheit, *Z. f. Psych.*, 45, 1907, 217ff. Benussi has modified his theories in later papers. A summary of his views will be found in Gesetze der inadäquaten Gestaltauffassung, *Arch. f. d. ges. Psych.*, 32, 1914, 396-419.

[20]E. Lindemann, Experimentelle Untersuchungen über das Enstehen und Vergehen von Gestalten, *Psych. Forsch.*, 2, 1922, 5-60.

figurational tendency, which may be said to express the most general law of configurational change: all phenomena of movement are strongly determined by the tendency of configurations to become as "good" as possible. While no precise definition of "goodness" is given, it seems to imply that configurations tend to become simpler, more stabilized, and to realize an optimal form.

A good configuration may be distorted by the introduction of a point near its border. Thus if a point is introduced into the periphery of a circle, the *gamma* movement is weaker at that point and the figure may be deformed; the circle becomes rectangular in shape. On the other hand, if the point is exposed alone, it appears in a different place from that which it would occupy if exposed in or near a figure.

The effects of fixation and attention are opposite: fixation destroys the movement, while attention favors movement in the part attended to. If the end of an ellipse is attended to, the direction of the movement is toward that end, "and seems to draw the rest of the figure after it." If the diameter of a circle is attended to, then the movement seems to be along that line. But when a part is *fixated*, the movement in that part is *poorer*. Fixation inhibits, while attention facilitates, the perception of movement.

According to Lindemann, it is evident that configuration influences the perceived movement, its direction, amount, and quality. The movement of a circle is stronger in its horizontal than in its vertical direction, although movement is present throughout the circle as a whole. But if we contrast the square with the circle we find that in the former parallel sides move outward and back, the vertical lines move more than the upper horizontal, and the lower line moves least of all. If however, the square is tilted on one corner, then all four angles "shoot energetically outwards and back as if it were a wholly different configuration." From this it appears that two directions are preferred, the horizontal and the upward, a fact which may have some bearing upon the anisotropy of visual space. Another fact of interest in these experiments came out when pointed figures were used: the movement in such figures was stronger toward the pointed parts than toward the rounded parts, a fact which is suggestive of the tendency of electricity to flow in the direction of a point. Figures drawn in perspective showed movement in the third dimension.

Several general properties of configurational phenomena emerge from Lindemann's descriptions. "Good" configurations tend to resist change and to preserve their form. If it is impossible for a figure to change in the direction of a simple configuration, a new configuration tends to appear. If the way to two configurations is equally short and easy, then the direction of the movement is uncertain and unclear, lacking univocal determination. Complicated configurations tend to change into more simple. The apparent size of a configuration is a function of the time of exposure: a certain minimal time is necessary for a configuration to reach its full size.

Many of the facts described by Lindemann received consideration and elaboration in Hartmann's work on the fusion of two figures exposed successively in a Schumann tachistoscope.[21] It is possible to change the time of exposure of each stimulus, the time between stimuli, the spatial interval between the figures, the intensity of light, the magnitude and form of the configurations. Fixation and other subjective factors were also taken into account. Although, as Hartmann points out, the phenomena of optical fusion had been studied at length, the earlier results led to no univocal and clear conception of the processes concerned in fusion phenomena. Nor were phenomena of fusion brought into relation with other facts, especially with those of movement and configurational tendency. Cermak and Koffka had already pointed out the relation between fusion and movement,

[21] L. Hartmann, Neue Verschmelzungsprobleme, *Psych. Forsch.*, 3, 1923, 319-397.

and had proposed a theoretical explanation of both which we shall consider later. Hartmann's research was therefore concerned not only with fusion phenomena but also with the way in which configurations arise, disappear, and change. His object was to ascertain the limen of fusion of two figures, either alike or different in form, and to note the effects of the spatial-temporal variables mentioned above.

It was possible to compare the fusion-points of figures like the triangle, circle, square and ellipse by determining the speed of rotation which would result in their fusion. The optimal relations of the variables for the fusion of various figures are best expressed in Korte's and Cermak's laws, which I shall next discuss. Here it suffices to point out that configurations possessing the simplest and firmest structures tend to fuse the quickest. On the other hand, if different figures are offered for fusion successively, each tends to preserve its own form. The law of constancy here prevails over that of fusion. Different figures of the same surface area and intensity possess different limens of fusion: thus the point at which a circle fused and was steady would give flicker when triangles of the same area were inserted. Different figures required different speeds of rotation in order to fuse. It thus appears, again, that the configuration as such is constitutive, exerting a real effect upon certain measurable phenomena.

Lindemann had shown that the configuration and *gamma* movement are closely linked together. Hartmann showed that the configuration is of importance upon the limen of fusion. Phenomenal configurations, even though illusory figures, exerted a real effect upon fusion, just as was the case in Kenkel's experiments on movement. Thus, if a figure-ground configuration was exposed, the configurations possessed different limens of fusion according as the one field or the other was taken as figure. When a field is ground it tends to fuse quicker than when it is figure, a fact which was made the basis of a special investigation by Gelb and Granit, and will be discussed later on.

The results of Hartmann's experiments may be stated thus. The effect of a configurational structure is more than summative, since the effects of mere geometrical or arithmetical increase in the size of the stimuli, the intensity of the light coming from them, and all other factors of importance for the phenomena studied, are to be explained only in the light of configurational changes and their influence upon the configuration as such. We may therefore say that simple and firm configurations tend to fuse quicker. On the other hand, configurations tend to maintain their form as opposed to radical change. The strength, direction and distribution of movement *within* figures depend upon a number of factors, chief of which is the configuration. The effect of any variable, indeed, depends upon the configuration: thus the factor of intensity has less effect upon "good" configurations than upon poor. In short, the limen of fusion depends upon the following factors as they influence the configuration: the intensity of the stimuli, the temporal relations of the stimuli, and the form of exposition. The detailed results bearing on the effects of surface *versus* outline configurations, and the breadth and position in space of the stimuli, can not be recounted here, nor the minor differences between Hartmann and his predecessors regarding the effects of fixation and the like. Hartmann's final conclusion regarding the fact that *alpha* and *beta* movement and the various stages of fusion are amenable to the same laws carries us back to the laws of Korte and Cermak upon which it is based.

We have already found that variations in the time of exposure, the spatial interval between successive stimuli, and the intensity of the illumination, all have an effect upon the kind of move-

ment perceived. Korte[22] undertook to vary some of the objective conditions, while the rest remained constant, in order to formulate the laws of dependence of movement upon the various factors which can be controlled in the stimulus-situation, on the assumption that the subjective conditions were for a time kept fairly constant. Korte hoped in this way to find an objective explanation for many of the facts which Kenkel had assigned to subjective causes. On the basis of the Wertheimer physiological theory, Korte was led to expect that it might be possible to compensate for the priority in time of the first stimulus by increasing the intensity of the second, and thus to determine the direction of movement. The four variables which Korte controlled were the absolute intensity or size of the stimuli, the difference in brightness of the stimuli, the time of exposure of and between stimuli, and the spatial distance between the stimuli. The effects of these variables were noted on the *beta* movement and on a fourth kind of movement which Korte called the *delta* movement, described as follows: if two stimuli (of which the one is more impressive than the other, owing to its greater magnitude, intensity, or variant form) are exposed in a Schumann tachistoscope, the stronger stimulus will move to the other stimulus; whereupon both may unite and return to the place of the first. The main characteristic of the *delta* movement consists in the return of the first stimulus to its original position, whether or not the second accompanies it.

The Korte laws applying to movement may be summarized as follows. "If the spatial interval is too great for optimal movement, then optimal *beta* and *delta* movement can be obtained by increasing the impressiveness of the stimuli (really their intensity); or by lengthening the pause between them. If the light intensity is too great, then optimal *beta* and *delta* movements appear with increase of the distance between the stimuli, or decrease of the time between their exposure. If the time is too great for optimal movement, one must either increase the interval between the stimuli or decrease the impressiveness. Optimal *delta* movement usually requires that the difference in impressiveness between the stimuli be increased."[23]

If we let s denote the distance between the stimuli, i the intensity of the stimuli, t the time interval between the stimuli, I the total impressiveness (*i. t.*), *opt.* optimal condition, and $\sim$ the rough proportional relation which exists between these various factors, we may state the laws of Korte for optimal movement as follows:

1. $s_{opt.} \sim i$	$i_{opt.} \sim s$
2. $i_{opt.} \sim I/t$	$t_{opt.} \sim I/i$
3. $t_{opt.} \sim s$	$s_{opt.} \sim t$

[22] A. Korte, Kinematoskopische Untersuchungen, *Beiträge z. Psychol. d. Gestalt*, 1919.

[23] *Ibid.*, 177.

In the formulation of these laws subjective factors were assumed to be constant.

It appears, Cermak tells us,[24] that the laws of Korte have a more general significance than a statement about apparent movement implies; for certain parallel phenomena, which were thought to be very different, are at bottom amenable to the same laws and can be referred to the same processes. Korte's experiments were limited to stationary objects and to two phases separated by a fairly large spatial interval. The question now arises whether the laws of Korte are valid when the interval between the successive phases of the stimuli becomes smaller or when more than two phases succeed one another. The assumption is made that, if the distance between the phases is reduced to zero, then the phenomena of apparent movement and light-mixture are identical, since light-mixture is a limiting case of successive phases of an object in which the distance between them is zero; *i. e.*, the phases come from a single source, and hence there is a temporal, but no spatial interval between them.

Cermak's initial assumptions have been critized by Hillebrand, who believes that the perception of light-mixture consists in an immediate awareness of several excitations which have been fused into one, and must be regarded as the perception of a single, simple datum which is not the correlate of the stimulus-components. The difficulty for Hillebrand lies in the fact that the stimuli under Cermak's conditions have the same spatial position, whereas in stroboscopic movement the stimuli are separated. For Hillebrand the two facts are different, both in origin and with regard to what is phenomenally perceived. But since Cermak's whole problem consists in discovering whether or not strobosocopic movement and the phenomena of fusion *fall under the same conditions*, it does not make any difference what is perceived so long as the same laws are responsible for both experiences. Hillebrand's objections do not touch Cermak's position.[25]

Let us return to Cermak's actual experimentation. A black belt 148 cm. long and 2 cm. wide revolved upon two cylinders. On it were pasted two rectangles cut out of white paper, 6 by 20 sq. mm. and 2 by 20 sq. mm. respectively. These figures, for purposes of comparison, were so placed that they moved at right angles to their length. The rotating belt and figures were illuminated from above by an electric lamp. The rays from the light-source were interrupted by means of a rotating disc. Sectors had been cut out of this disc, leaving openings of different sizes through which any amount of light could be transmitted at any frequency. The speed of rotation of the belt could also be varied either with, or independently of, the light-frequency. The stimulus thus consisted of two white figures on a black belt moving at different rates (at different times) and illuminated by flashes of light whose frequency varied. It will be seen that here, in contrast with Wertheimer's work, it is not a question of two stationary stimuli, separated by a spatial-temporal interval, giving rise

[24] P. Cermak and K. Koffka, Untersuchungen über Bewegungs- und Verschmelzungsphänomene, *Psych. Forsch.*, 1, 1921, 66-129.

[25] F. Hillebrand, Zur Theorie der stroboskopischen Bewegungen, 1, *Z. f. Psych.*, 89, 1922, 209-272, and 2, *ibid.*, 90, 1923, 1-66. Wertheimer has answered Hillebrand on his criticism of the "short-circuit" theory in *Psych. Forsch.*, 3, 1923, 106-123. See also Hartmann's treatment of the Hillebrand criticisms in *Psych. Forsch.*, 3, 1923, 370ff.

to the perception of different kinds of movement. In Cermak's experimentation, by change of the variables (frequency of light flashes, speed of rotation of the belt, *etc.*), the *O* sees: (*a*) a single moving object, (*b*) an object broken up into several objects which are moving simultaneously, or (*c*) several objects in stationary succession. The assumption is made that, when the belt is not rotating, *i. e.*, when the speed is reduced to zero, and the distance between the stimuli is zero, we have the limiting case of apparent movement, since the decrease in the distance between the two stimuli has reached the lower limit of spatial-temporal separation.

We may briefly summarize Cermak's results as follows. If we have a speed suitable for optimal movement, an increase of speed destroys it, and gives the perception of several objects, similarly to the simultaneous stage of apparent movement.[25] This phenomenon may be called flicker. This kind of flicker, however, must be distinguished from the flicker arising when one object appears stationary in successive positions. If the speed of the belt is decreased, one object appears at rest in different places, corresponding, in some ways, to Wertheimer's successive stage.

Korte's laws were found to hold, except the third in the case of large light-frequencies. The rates of speed of the rotating belt during the time that optimal movement is still seen are, however, larger than they should be according to Korte's law. According to Korte, when optimal movement appears with any frequency v and any speed of exposure c, a lessening of c should result in succession; in Cermak's experiments, the moving object should appear in various positions without movement. It should do this for any frequency, therefore for every value of v. Cermak found that this is true only for low values of v, and only when the illumination-intervals are long does one see discontinuous succession. If the frequency v is large, then, in spite of the decrease in the value of c, the movement remains optimal. One must therefore decrease the light-frequency a great deal in order to get succession. This result seems to show that succession is due to long intervals between the light-flashes and not to the small spatial distance between the stimuli, a fact which is demanded by the Wertheimer and Korte theories and laws.

To correct this defect, Cermak introduces the "zone" law to take care of all possible cases like the above: when v is large, the range of values of c grows in such a manner that the upper limit remains almost unchanged; v is then the value of the critical fusion-frequency. Further increase of the frequency increases the values of c positively. In this way the transition is made from optimal movement (which gave the most trouble) to light-mixture.

Cermak formulates eight laws, which are known as the parallel laws, applying to light-mixture and apparent movement. They express the dependence of the variables governing phenomena of fusion and apparent movement, and may be stated as follows: (1) in order for fusion and simultaneity to appear the light-intensity must vary inversely with the time of exposure; (2) other things equal, lessening the time of exposure favors the fusion and simultaneous stages; (3) changing the time of dark exposure influences fusion and movement more

[25] In other words, if the movement of an object is too rapid it loses the appearance of movement and a streak of light is seen motionless. The simultaneous stage of several objects at rest is perhaps related to the multiplication of objects in the moving picture when too rapid rotation results in duplication of the seen object, but all are stationary images. *Cf.* K. Koffka, *The Growth of the Mind*, 37 and note 51.

than changing the period of illumination; (4) the greater the time of exposure, just so much the greater must the pause be between the phases if any kind of movement or any degree of fusion is to be seen; (5) making the object smaller favors the simultaneous and fusion stages; (6) central vision favors the appearance of simultaneous movement and fusion; (7) increasing adaptation to dark vision favors the simultaneous movement and fusion, except for very weak intensities; and (8), up to a certain limit, lengthening the period of illumination hinders the fusion and simultaneous phenomena.

These parallel laws, Cermak believes, force us to assume a common set of underlying causes for the phenomena of fusion and of apparent movement, and a theory must be constructed embracing both. Since Lindemann and Hartmann, particularly, have shown that the phenomenal configuration influences both the kind of movement perceived and the limen of fusion, it becomes clear by implication that the facts of movement and of fusion at the same time express certain facts of configurational change. Keeping this conclusion in mind, we may say that the configurational processes underlying movement and fusion are similar, if not identical. In order to justify this inference, Cermak takes over Müller's psychophysical axiom which asserts that identity, similarity, or difference in any phenomenal process corresponds to an identity, similarity, or difference in the psychophysical process.[27] Hence, if we find two phenomenal processes that are alike or obey the same laws, the brain-processes underlying them must be correspondingly alike. The phenomena of light-fusion and movement, we may conclude, arise from a common set of psychophysical processes. And it would seem to be a necessary corollary from the development in theory which we have been tracing that, if fusion and movement are alike and simultaneity and fused quality are similar, then it is highly probable that quality and spatial form may have the same underlying causes if they prove amenable to the same laws. A fact which seems to point in this direction appeared in Hartmann's experiments, where it was found that changing the color of the figures exposed for fusion had practically the same effect as changing the form of the configurations: "verschiedene Farben verhalten sich also bei gleicher geometrischer Reizkonfiguration hinsichtlich des Flimmerns anscheinend ähnlich wie verschiedene optische Raumgestalten."[28] If it is found that the facts of quality can be brought under the laws governing spatial-temporal configurations, thus correlating quality with time and space, then one of the most

[27]G. E. Müller, Zur Psychophysik der Gesichtsempfindungen, *Z. f. Psych.*, 10, 1896, 1-2.

[28]Hartmann, *op. cit.*, 377.

difficult problems of experimental psychology seems to be well on the way toward a solution.

Another investigation into the effect of time upon spatial extents and of both space and time upon the *beta* movement shows a further development of the configurational concepts to include within a given configurational phenomenon several additional variables which may affect the experience. We find in these experiments, conducted by Scholz,[29] that the same laws govern these phenomena in three different sense modalities, vision, hearing and touch. Benussi and Gelb had already found that two spatial extents defined by three lights flashed successively may be made to appear longer or shorter by varying the time interval between the stimuli.[30] The spatial interval between two light flashes seems to shorten when the time between them is decreased. The question arises, what interval between the successive flashes will give the shortest spatial extent. The whole problem is complicated by the fact that phenomenal movement appeared during the optimal time for the maximal shortening. We find thus that movement, extent and time are related, not only in vision, but in hearing and touch as well.

The method used by Scholz was different from that used in the Korte experiments; yet Korte's laws on the whole were verified. Standard intervals were fixed by three lights constantly illuminated. Above the standard lights were the intermittent lights whose flashes defined the spatial extents to be compared with the standard distances. The experiments in hearing consisted in giving two sounds whose sources were separated by varying distances. The *O* was asked to indicate how far apart the two sounds were by showing their positions on a meter stick parallel to the sources. In these experiments the *O*s reported an auditory phi phenomenon. The tactile experiments were made by stimulating the *O* with successive touches at different times and at different places. The *O* pointed to the spot last stimulated; and here also the shorter temporal intervals gave the impression of a smaller spatial extent on the skin. Movement and extent were also closely related in this sense modality.

In all three sense domains it was found that the phenomenal magnitude of a spatial extent depends upon the temporal interval between the successive stimuli. When the real distance between the stimuli is sufficiently large there are temporal intervals in which the magnitude of the phenomenal extents becomes considerably smaller than the normal. The normal extent is defined as the distance between any two stimuli when they are presented simultaneously. Moreover, the optimal temporal interval for the maximum of shortening coincides with the time in which apparent movement appears. It would therefore be safe to say that movement and time and space are mutually related, at least within certain limits; outside these limits we must note certain other effects.

With small separations between the stimuli, shortening of the apparent extents increases with the smaller temporal intervals below the zone in which movement takes place. Below the limen for movement, fusion of the stimuli is reported and only one stationary sound or touch appears, a fact which may be related to the Cermak parallel laws. If the distances are very small, the apparent spatial extents seem greater. In vision this result coincides with the appearance of movement, just as the greater real distances give apparent movement with the maximum of shortening. In

[29] W. Scholz, Experimentelle Untersuchungen über die phänomenale Grösse von Raumstrecken, die durch Sukzessiv-Darbietung zweier Reize begrenzt werden, *Psych. Forsch.*, 5, 1924, 219-272.

[30] V. Benussi, *Psychologie der Zeitauffassung*, 1913, 285 ff., and A. Gelb, Bericht über den VI. Kongress für experimentelle Psychologie, 1914, 36 ff., 159 ff.

hearing and touch the zone in which the phenomenal extents increase with longer temporal intervals was found to extend into the successive stages of movement. All three stages of Wertheimer's phi phenomenon appeared in these experiments. In the experiments on hearing and touch, the phenomenal spatial extent decreased with the decreased temporal interval to the point where *equal* objective extents appeared phenomenally greater or smaller, if the temporal interval between the stimuli was increased or decreased. Movement accompanied both increases and decreases of extent.

Localization of tactile stimuli is very labile and uncertain, even when the temporal intervals between the stimuli leave no uncertainty as to their position. The order or direction of the stimulations is of importance here because there is a tendency to make errors in the proximal direction. If the order is proximal to distal, the distal spot is displaced more; for owing to the tendency to localize proximally, the distal is affected more than the proximal when the order is proximal to distal, since the spots summate. When the order is distal to proximal, each works against the other and compensation lessens the amount of error.

We have thus traced the development of configurational theory in the field of movement and have found that phenomenal movement can be related to other classes of configurational events and is subject to the same laws. The tendency of configurational experimentation seems to be toward extending configurational principles to the various sense modalities, unifying as many different fields as possible, and reducing apparently diverse phenomena, like movement and fusion, spatial extent and movement, form and quality, to more simple underlying configurational processes.

(13) *Configurational Physiological Theories of Movement*

"The main thesis of the configurational theory may be said to be that physical configurations of the nervous system possess properties parallel to their phenomenal correlates."[31] This quotation from Hartmann shows beyond all peradventure that the configurationists regard as the most important part of their theory that part which forms a bridge between psychology on the one hand and physiology and physics on the other. The considerations leading to this position can be stated briefly. By means of physical and physiological hypotheses the configurationists have been able in many cases to control and predict the phenomena which they are studying. It is almost a truism that we have no laws in psychology that can be called psychological laws pure and simple. The so-called psychological laws, *e. g.*, association, color mixture, apply really to stimuli. There are few theories, either, of the physics or of the physiology of the nervous system which are adequate to account for more than very restricted kinds of psychological process, *e. g.*, the color theories of Young-Helmholtz, Hering, and Ladd-Franklin. Most extant psychological theories, if they possess any value in

[31] L. Hartmann, *op. cit.*, 391.

explaining the phenomenal processes, furnish no hints as to the processes which accompany them in the nervous system, just as the physiological theories throw no light on the phenomenal pattern.[32]

The aim of the configurationists, in formulating their physiological theories, has been to account for phenomenal patterns by means of configurational processes in the nervous system without any break in theory. If the wholes, patterns, formal aspects of experience cannot be built up out of summative elements, then it becomes necessary to postulate configurational processes within the nervous system to furnish a physiological correlate for the phenomenal data. Moreover, if configurational principles can be applied to the physical processes at the basis of phenomenal and stimulus patterns, the same laws may be formulated for both. Then, by controlling the objective situation according to laws of configurations, we may be in a fair way to control the experiential processes. It is possible to control the objective configuration, by changing spatial-temporal and qualitative dimensions of the stimuli. What goes on in the nervous system must be a matter of conjecture, largely, especially as regards the actual brain-processes involved in perception, thinking and acting. Configurational theories of neural action have therefore been avowedly heuristic.[33] They have been advanced to stimulate further research by furnishing hints for functional variations in conditions. Wherever possible, the configurationists have tried to substitute configurational laws, in terms of objective principles applying to the nervous system, for the subjective factors usually found in psychological theory.

It is necessary to begin with the first configurational hypotheses if we are to understand the rôle that physiological speculation has played in the configurational movement. It must be borne in mind that, in view of the later experimentation, a large number of new facts embracing real movement, apparent movement, colored movement, fusion and the like must all be amenable to whatever theory is finally accepted.

Wertheimer's original theory was designed to explain the facts of apparent movement without resorting to associational-

---

[32]It will be remembered that the configurationists recognize the problem of the gap between mind and body and endeavor to bridge it by means of the configurational principles, building psychology upon the foundations of physics.

[33]To those who believe that explanation of mental process is to be found only in the nervous system, this part of the configurational programme should strongly appeal. On the whole, configurational over-emphasis upon the nervous system is a reaction against an extreme phenomenology which has crept into German experimental psychology in the last few years. For the extreme phenomenological method, see P. Linke, *Grundfragen der Wahrnehmungslehre*, 1919. *Cf.* Note 39 below.

atomistic physiological assumptions.[34] The brain process corresponding to the movement must be a total, structured configurational event, areal in character. Such processes, Wertheimer assumes, exist, and may be described in the following manner. If a place *a* is excited, a circle of excitation arises spreading from *a*. If two places, *a* and *b*, are excited, the circular spread occurs from both. If *a* is excited before *b*, a short circuit takes place: the spread of excitation crosses from *a* to *b*. The direction of the current is determined by the part first stimulated. The nearer *a* and *b* are to each other, the more favorable are the conditions for the phi phenomenon.

The successive optimal and simultaneous stages of movement are explained as follows. (*a*) Successive movements of two lines presented stroboscopically occur when the time between the exposure of the one, *a*, and the other, *b*, is so great that the circular activity of *a* is extinguished before *b* is stimulated, and hence *a* moves and stops before *b* appears and moves. (This explanation does not account for dual part movement, when both stimuli move at once alone.) (*b*) Optimal movement (*a* moves over to *b*) occurs when the time between *a* and *b* is just long enough for the excitation from *b* to meet the excitation from *a* at its height. (*c*) The simultaneous stage (both *a* and *b* appear simultaneously at rest) occurs when the time is so short between *a* and *b* that the circular excitations take place immediately and do not reach a sufficient height to make possible a short-circuit between the two.

It is also assumed that no part of the excitations in the brain reaches consciousness before the total does. The single stimuli must give rise to excitations which "spread" before becoming conscious,—a point which Köhler later tries to explain. But it is not clear whether *a* and *b* act as isolated stimuli in the successive stage, where each moves alone. As a concrete illustration will show, the influence of the single stimuli is left in doubt.[35] Let us take the perception of a triangle whose sides are *a*, *b* and *c*. According to the theory outlined above, since all the parts are symmetrical, they must influence each other in equal amounts. In the case of a patient who was unable to see a triangle and was obliged to judge its form by passing his fingers around the contour, time was needed to travel from one side to the other, as in Wertheimer's successive case. If *A* represents the circle of excitation in the brain of side *a*, *B* of *b*, and *C* of *c*, then *A* must influence *B*, *B* must influence *C*, and *A* must influence *C* equally,—*i. e.*, *AB* must be equal to *BC*, *CB* must be equal to *CA* and so on. In other words, the relations between *A* and *C* must have the same effect in the total picture as *A* and *B*, and *B* and *C*. But this result is precluded by the temporal inequalities; by the time the patient's hand has reached side *c*, the excitation initiated by *a* has less influence on it than *b*. The Wertheimer theory implies that symmetrical spatial configurations are perceived when their parts have proportional excitational spreads. In the case cited, the time interval made no difference, and the relations of the sides to one another were not mutually reciprocal; yet a symmetrical figure was perceived. The theory encounters difficulties also in assuming that discrete stimuli may give rise to unitary structures. Nor can one assert that the neural configuration must be completed before the phenomenal configuration appears, because the parts may be perceived as

[34] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Z. f. Psych.*, 61, 1912.

[35] Taken from K. Gneisse, Die Enstehung der Gestaltvorstellungen, *etc.*, *Arch. f. d. ges. Psych.*, 41, 1921, 295-334.

such and yet be recognized as belonging to a single configuration. On the other hand, if the parts become conscious before the brain configuration is completed, the theory falls; for it demands that the consciousness of the whole is possible only when the various single excitations meet in a unitary structure.

Further elaboration of the Wertheimer theory has been undertaken by Koffka. The relations among the central processes responsible for the various kinds of movement are not given by Korte's laws. Koffka asks where the two excitations from *a* to *b* meet, when they meet, and how strong each is at the place and time of meeting.[36] By geometrizing the central neural processes arising as a result of the stimulus excitations, Koffka arrives at a mathematical formulation of the laws of Korte based on the Wertheimer theory. Here again the case of successive movements of each stimulus offers difficulties; for here the one stimulus may take effect without the other, and discrete processes are the correlates of the movements, whereas in optimal movement it was demanded that the effect of the stimuli be regarded as "total, unitary processes." On the other hand, the fact that the *second* stimulus may be of influence upon the first requires a more elaborate theory than the simple hypothesis advanced by Wertheimer.

In order to explain how discrete stimuli can influence each other and how they can give rise to the various phenomena of movement and fusion Hartmann and Köhler have amplified the original Wertheimer theories.[37] Since Köhler's views are a restatement of the Hartmann theory, let us consider Köhler's theory.

Wertheimer found that, even though the stimuli *a* and *b* were colored, it was possible to get movement in which no trace of color was visible. But he also found that his *O*s obtained colored movement. Köhler asks: "How do two stimuli, *a* and *b*, give one movement, and how can two stimuli give colored movement at one time and at other times movement which lacks all traces of color (pure phi)?" Recognizing that discrete stimuli are a source of difficulty, and that the short-circuit theory needs revision, Köhler assumes that the optic sector reacts as a whole to the whole constellation of physical stimuli. The incoming excitation currents are functions of the total situation and can be described in physiological terms as follows: if *A* is the central nervous process corresponding to stimulus *a*, and *B* that corresponding to *b*, when *a* and *b* are presented either simultaneously or near one another, *A* and *B* are different from what they would be if each occurred alone. Therefore *A* is determined not only by *a* but also by *B* within the central nervous system. In this way illusions can be explained which are due to *Nebenreize*.

The way in which *A* and *B* influence each other finds a different explanation from that which Wertheimer had employed. Köhler asserts that the stimuli *a* and *b* set up currents in the optic sector with different voltages. *A* and *B* fuse into one

[36] K. Koffka, Zur Theorie einfachster gesehener Bewegungen, in *Beiträge z. Psychol. d. Gestalt*, 1919, 283-318.

[37] L. Hartmann, *op. cit.*, 391 ff; W. Köhler, Zur Theorie der stroboskopischen Bewegung, *Psych. Forsch.*, 3, 1923, 397-406.

beneath the psychophysical level, so that the conscious process arises as a unitary structure. In this way Köhler meets the objections raised above against the Wertheimer theory. Both currents tend toward an equilibrium. Korte's results confirm this theory better than the Wertheimer theory; for he found it possible for a later stimulus to be perceived as earlier in time, if it possessed a greater intensity than the first.

We may call this a theory of "sub-psychophysical absorption" to account for the various stages of movement and fusion. The part, successive and simultaneous stages are easily accounted for in the following manner: if *A* and *B* do not fuse, then part movement of each is seen; if *A* and *B* appear too closely together, both are seen at rest.

To account for the presence and absence of color in movement Köhler has recourse to his earlier theory of color which found expression in *Physische Gestalten*. In order for color to appear in consciousness, it is necessary for unequal amounts of ionic concentrations to pile up in various parts of the optic sector. If this damming process does not occur, color will not appear. Hence time is necessary for the perception of color. If the displacement of ionic concentrations is not too fast, color and movement will both be perceived. If the concentrations do not reach a sufficient strength, no color will be seen and a pure movement will be the result.

The theory, thinks Köhler, explains the physiological bases of movement in the various sense domains. If two pressure sensations are felt together, they seem to be nearer than if separated by a greater interval of time. If a strong and a weak stimulus are presented, the stronger will tend to absorb the weaker. Some of Benussi's results seem to harmonize with this theory; for it was found that tactile *beta* movement seemed to grow smaller when the stimuli were moved together in time until only one sensation was felt. In the domain of vision it was found that, the shorter the time of exposure between any two lights, the nearer they seemed to be in space. The same thing holds for sound.[32,33]

---

[32]Benussi found the phi phenomenon on the skin, getting movement not only on one arm but from one arm to the other through the air. Lack of space forbids a discussion of his results. His work on tactile phi is contained in several articles, a summary of which will be found in Versuche zur Analyse taktil erweckter Scheinbewegungen, *etc.*, *Arch. f. d. ges. Psych.*, 36, 1917, 59-136. *Cf.* Scholz' work, discussed in the preceding Section.

[33]Linke, *op. cit.*, has criticized the configurational theories of movement on the ground that they do not explain the perception of movement by reference to purely psychological principles. He asserts that we must find an explanation of movement within the phenomenal facts themselves, since an appeal to the nervous system throws no light on the experience of movement. Linke's theory of movement does not take account, therefore, of the stimuli, the nervous system, or the origin and changes of movement due to physical or neurological factors. His position is purely phenomenological, and shows the psychological influence of Husserl at its most extreme. Linke regards movement as a function of "assimilative perception," which is not to be confused with Wundt's concept of assimilation. When

(14) *Formal Spatial Effects Due to Configurational Properties*

We have already seen that different phenomenal configurations possess different phenomenal properties by which they may be distinguished, *e. g.*, such general differences in clearness, persistence, "thingliness", and impressiveness as are denoted by the concept of figure-ground. While it is difficult to draw a sharp line between descriptive and functional concepts, we may regard the figure-ground properties as descriptive, while the effects of the configuration upon the perception of movement, the limen of fusion, and the like, are functional. But the configuration may exert a functional effect upon phenomena which is difficult to characterize as either static or dynamic *per se*. Among these borderline phenomena may be reckoned stereoscopic vision, after-images, the perception of depth and of convex and concave objects, and many of the facts we shall discuss in following Sections. In this Section we shall be limited to the effects of configurations as they appear in the perception of space and in allied experiences.

(*a*) *Stereoscopic Vision*

Lau, who has attacked the problem of stereoscopic vision and depth from the configurational point of view, is concerned to prove that stereoscopic vision depends upon more than a simple functional interplay of retinal elements.[40] Lau showed his *Os* 6 parallel lines in the one field of the stereoscope and the Zöllner figure in the other. Lau asserts that, if the peripheral, atomistic explanations of stereoscopic vision are correct, we should expect the parallel lines to lie in the same plane with the Zöllner figure as when offered alone; for nothing has changed, according to the element-theories, in the parallel-line image on the retina. But if the images from the two eyes form a single configuration, and if, as we should expect, each field is a different configuration, then the parallel lines must appear in a more complicated spatial arrangement when offered with the Zöllner figure. Observation proves that the parallel lines appear to be in a different plane from that in which the criss-cross lines are seen.

---

only part of an experience is given in perception, and this part has the compelling power to re-instate or complete it, then assimilative perception is present. Pre-perceived elements enter into the given perception and complete the sensory data. Movement consists of a succession of phase-images or a plurality of place-determinations. To be the phases of one movement means to be parts of one and the same object. What moves is the thing which is identified in the various phases. Linke regards this explanation as a true descriptive explanation, without an appeal to the stimuli or the nervous system. But a single instance is sufficient to show the weakness of the theory: Gelb and Goldstein found a patient who was able to see a moving object only in successive places, each phase being stationary. This patient knew that the phases belonged to a single object, and identified them as the same, yet he saw no movement. How can this observation be explained on the basis of Linke's phenomenological theory? *Cf.* A. Gelb and K. Goldstein, *Psychologische Analysen hirnpathologischer Fälle auf Grund von Untersuchungen Hirnverletzter*, 1920, 96.

[40]E. Lau, Versuche über das stereoskopische Sehen, *Psych. Forsch.*, 2, 1922, 1-4, and Ueber das stereoskopische Sehen, *ibid.*, 6, 1924, 121-126.

(b) *Depth, Rivalry and Related Phenomena*

Experiments in which the Zöllner lines crossed the parallels in both fields, except that the lines extended in opposite directions for each set of parallels, showed that rivalry set in: the parallel lines appeared in the same plane while the Zöllner lines appeared in different planes, depending upon which image dominated. The illusion was also increased. Köhler, in preliminary experiments with the Müller-Lyer figure, reported negative results, and Lau found that this figure was destroyed when he tried the experiments described above. In a later paper,[41] Lau has elaborated a configurational theory of depth perception on the basis of further experimentation. This theory embraces the facts just cited. Lau declares that it is not merely the disparate images which are responsible for depth; depth, he believes, arises as a result of the deviations between configurations; retinal points do not act in isolated fashion, but the configurations are effective as total structures. Qualitative considerations also are involved in the perception of depth; for a white line on a black ground, given as one field, does not fuse with a white surface, given as the other field, even though identical points are stimulated. In stereoscopic vision there is an "equalization" (*Vergleichung*) of structures, and this process may act reflexively upon the structure of each retinal image. Thus monocular structures can be destroyed when the configurations offered to each eye are too unlike: the different parts of each image come into rivalry, with the result that similars fuse and not a trace of the original remains; or parts of monocular structures may be unified into a single configuration, and each configuration may be put in a different plane, as was shown above.

The phenomena of rivalry in connection with the theory of identical and corresponding points have been further investigated by others whose results support Lau's configurational theories. Let us take first Lewin and Sakuma's experiments on movement and depth.[42] These *Es* presented two cards with figures on each, one to each eye, in the Hering haploscope, and moved either all or parts of the figures. The figures were such that both eyes might have images in common (binocular images) or one figure might contain parts not present in the other (monocular image). With movement that is too fast, double images result; with the right speed (moving only one image, usually the left), the mono-image hurries on in advance of the rest of the configuration which both eyes have in common. The monocular and binocular images give different quantitative results when measured with respect to movement, rate of movement, and the like.

It is interesting to note that real movement of part of a configuration presented to one eye could cause phenomenal movement or distortion of parts of the objectively stationary binocular structures. Thus if two vertical lines, composed of three black dots separated by a small interval, were presented to both eyes, and if the top dot of the left field was moved to the right, the result was that (instead of a vertical line with only the top dot displaced to the right) the middle dot moved half as far as the top dot (only phenomenally, of course), and the resulting figure was an oblique line whose direction was to the right, the bottom dot forming the base of the oblique line without having stirred from its position.

More important still is the fact that disparate images may give single structures, while images on corresponding or identical points may give double structures. The less deformable a figure is, the easier may it double and give rise to double images.[43] If two triangles having a common base

[41]Lau, *op. cit.*

[42]K. Lewin and K. Sakuma, Die Sehrichtung monokularer und binokularer Objekte, *etc.*, *Psych. Forsch.*, 6, 1924, 298ff.

[43]H. Hildebrandt has reported on the changes in configurational structures in astigmatic vision. See Experimentelle Untersuchungen über das Sehen bei nicht-optimaler Akkommodation, *Psych. Forsch.*, 6, 1924, 112ff.

and apexes pointing in different directions are presented, they are on identical points of both retinas and yet are seen as two. *Retinal points can thus be regarded rather as retinal zones which will suffer a certain amount of displacement before double images appear.* It was found that, to prevent accommodation to abnormal convergence, a displacement of retinal zones did occur. When one of the images or a part of it is moved, "simple" vision is impossible unless there is some distortion of retinal ordinates; since we are dealing with labile systems, not with absolute points, it is possible to preserve a unitary phenomenal structure when non-corresponding points of the retina are stimulated.

The question now arises concerning depth: if identical points on the retina are fixed, why does not depth arise when the left arm of the haploscope is moved? Again, since the binocular images are not on identical points (for we must assume in the cases cited above that there are disparate images on the retinas), why does the *O* not get depth relatively to the monocular points? There is some depth effect, but it is minimal. Lewin and Sakuma advance a provisional theory of monocular depth, according to which depth arises when the retinal zones in one and the same eye undergo different and opposite strains (*Durchbiegungen*). Or, in other words, a relative depth effect appears when, in the transition from a stable motor system, the strains or tensions do not disappear. It is admitted that other factors like intensity and eye-movements are of importance; but the essential point of this configurational theory lies in its insistence that the changes within a single eye depend upon the deviations between the retinal images of both eyes! Each retinal image is affected by the image on the other retina.

Closely related to the work of Lewin and Sakuma is Kuroda's investigation of binocular phenomena.[44] When the eyes are stimulated by different light-rays any one of three things may happen: one image may dominate, rivalry may set in, or there is a fusion of both fields. The quantitative and qualitative differences in the stimuli determine what shall happen in the retina. So far as quality is concerned two factors are of importance: the intensity of the stimuli, and the amount of similarity existing between them. Thus contours possess greater dominating powers than their fields; but, if they are broadened, they lose their significance and also their dominating qualities. Different forms possess different grades of domination. Different colors possess different degrees of dominating power; yellow more than blue, white more than black, red more than blue, and so on. The greater the difference between the two stimuli, the greater the domination of the stronger.

If figures are alike, simple vision is assured. On the borderline stand figures which are unlike but similar in quality. Mixture may take place between white and black, for example, when the brightness difference is reduced. Domination and mixture are inversely related. Other variables which may affect binocular vision are brightness, saturation and the size of the single fields. In general, various amounts of these factors affect mixture, as other investigators have already sufficiently demonstrated.

Kuroda's conclusions are that domination and rivalry appear only when there is a sufficient amount of difference between the stimuli. But rivalry appears when the difference between the stimuli is not too great; rivalry is a half-way stage between mixture and domination. In reality, rivalry is *alternate* domination, and is related to domination in that it requires a low degree of difference. Rivalry is also related to mixture in requiring some degree of similarity between fields.

---

[44] G. Kuroda, Zur Grenzbestimmung der binokularen Phänomene, *Psych. Forsch.*, 6, 1924, 282-297.

(c) The Perception of Two Colors, the One Behind the Other

Kuroda does not mention a point, which might well have come up in his experiments, concerning the fact of the simultaneous appearance of two colors, the one behind the other. This has been a moot question since Helmholtz gave an affirmative answer and Hering denied that it is possible to perceive two colors simultaneously, the one behind the other. Stated more precisely, the point at issue is whether identical retinal spots in monocular vision can mediate the perception of two different colors at the same time and result in the perception of depth. The problem has been investigated by Fuchs, who found that different colors may appear simultaneously in different planes behind each other. He says that "the conditions for the appearance of this phenomenon are the presence of suitable configurations in configurational perceptions. The main condition is that both objects, the transparent as well as the object seen behind it, must be perceived as two different, complete configurations."[45]

Fuchs used a piece of glass tilted at a suitable angle with the one color in front and the other behind, so that the *O* saw the color in front reflected from the side of the glass toward him, while the other color was perceived through the glass. Either color may vary in size. While it is difficult to perceive one color behind the other, Fuchs was able to find enough positive *O*s to get very interesting results. It is more difficult in monocular than in binocular vision. If one color is regarded for itself, there will be no transparency, no depth; for either a single color will appear which is a mixture of the two colors, or else only the color fixated. If single points on either colored surface are fixated, either one color or a mixture is seen. The same thing is true if the contours are fixated. If the figures are regarded as wholes, one can attend to single points or contours and still get depth, but in general point-fixation destroys depth.[46]

It is not a question, Fuchs tells us, of noticing now one color and now another; both are seen simultaneously, in different planes. Fuchs concludes that the perception of depth is an immediate experience, requiring no more mediation than the perception of right, left, up and down. In agreement with Lewin and Sakuma's later work, quoted above, it is possible in monocular vision to get depth and also two colors in different planes. It is thus not even necessary to stimulate different retinal spots in a single eye in order to perceive depth monocularly.

If one observes with one eye and then uncovers the other, the farther color shows a displacement backwards. Eye movements and head movements increase the transparency of the color seen in front and the clearness of the color behind. The same effect can be achieved by moving the colored objects. The explanation advanced for the effects of movement is that movement helps to bring out the form of each configuration and thus accentuates the color of each. It is possible to invert the colors, so that what was seen before may now be made to appear behind the other. The *O*s reported that the depth factor is increased when the stimuli are regarded as objects and the sensations of color are neglected. Colored gelatine or smoked glass as the reflecting medium destroys the perception, because the surfaces cannot be surveyed and regarded as structures *per se*.

With yellow and blue circles, the yellow in front of the blue, if the yellow is subjectively localized in the same plane as the blue, it will appear

---

[45]W. Fuchs, Experimentelle Untersuchungen über das simultane Hintereinandersehen auf derselben Sehrichtung, *Z. f. Psych.*, 91, 1922, 146ff. Many of the facts described by Fuchs from the configurational point of view have received attention from members of other schools. Katz must be mentioned in this connection, as his book on color vision has been a source of information to Fuchs. See Note 6 for a reference to Katz' work.

[46]Fuchs, *ibid.*, 161. I omit discussion of the experiments with *Spiegelbilder*.

whiter or grayer. If it is localized behind the blue, it looks more yellowish than when seen in the same plane, but less yellow than when localized in front of the blue. When the yellow is objectively behind the blue, if it is localized in front of it, it appears more compact, a better yellow and more "thingly," than when seen where it really is. Fuchs concludes that yellow cannot compete with blue as a color because it lacks chroma, compactness and "thingliness" as compared with blue. Yet, as we saw above, Kuroda found, at least in the case of one observer, that yellow dominated in the stereoscope when presented with blue.[47]

Experiments with different stimulation of identical retinal spots also resulted in the appearance of one object lying behind another. The double image method was here used: a colored surface and then an object were presented to one eye, and it seemed as if one were looking at the surface behind the color of the object.

The influence of the configuration upon the colors may be summed up, according to Fuchs, in this way: the form of the contour is important in determining whether the colors will be seen as wholes, or on a background, or in parts; the configuration of the surface may be so conditioned by the contour that it is less unified than it might otherwise appear. Parts of the surface may disappear altogether or appear as parts of another configuration; the configuration may be such that a spot will raise itself above the rest of the surface, appearing as a whole in its own right, while the rest of the configuration forms the background or surroundings for this independent bit.

Shadows played some part in the experiments; they deserve mention, although Katz has described similar phenomena in his work on colors. The colors in Fuchs' experiments often looked shaded. But differences in the shading were noted; there are opaque and transparent shadows. When the shadow is opaque, it is in front of (or on) the colored surface, but does not form part of the configuration. When the shadow is transparent, the color of the surface can, of course, still be seen behind it. The transparent shadow seems to be the limiting case of double color perception,—the distinction between lighting and that which is lighted (*von Beleuchtung und Beleuchtetem*) seems to be the last stage in separating colors.

All of Fuchs' *Os* with the exception of one (and at times another) agree that the phenomena described consist of an immediate experience of two colors behind one another and in the same direction. They deny that the perception is a matter of meaning, since it is constituted by an immediate sensational content *sui generis*. It is not a question—*contra* the views of the members of the school of the form-quality—of something new which results in a new content added to an already familiar experience. The essential conditions of these phenomena, Fuchs believes, reside in the configurational attitude or perception.

It is difficult to draw any general conclusions or to trace any special development in configurational theory from the work on binocular vision and the perception of depth. There does seem to have been an attempt made to determine what occurs inside the configuration under the varying conditions. In the work of Lewin and Sakuma a theory is advanced to account for the

[47]There are also differences of a more important kind among *Os*, as Fuchs and others have discovered. Some of Fuchs' *Os* were unable to see two colors simultaneously, the one behind the other. Fuchs classifies these as the "analytical" *Os*, since a configurational or totalizing attitude was necessary to get the phenomenon. Similar results with analytic *Os*, where configurational phenomena are in question, have been obtained by Ackermann, Eberhardt, Benussi and others.

perception of depth in monocular vision. Fuchs had left his facts in the air, after ascribing them to an "immediate awareness". The later theories of the configurationists represent an attempt to discover the mechanisms, physical and physiological, which are responsible for the phenomenological data they have described. It may help us to understand these attempts better if we summarize briefly the configurationists' general position with regard to space perception.

(15) *Configurational Theories of Space Perception*

The configurationists reject both the nativistic and the genetic accounts of the perception of depth, localization, orientation and direction in space. Phenomenologically, space-perception is an immediate experience of certain properties possessed by objects in the external world. Genetic theories, which attempt to derive space from elementary sensations which either do not possess spatial characters at all or are endowed with "local signs", err in assuming that mind builds space out of non-spatial elements or that "experience" can form spatial configurations by summative processes. The nativistic theories are attacked because they neglect the objective conditions under which the perception of space is made possible; for even though configurations are immediate data of experience, they arise only when conditions favor structuration. Phenomenal space copies real space, often in spite of the anisotropy of the retina and optic sector; the deviations between phenomenal and real structures must be explained, in so far as we are able, in terms of the physics and chemistry of the nervous system. We must also take into consideration whatever subjective factors we find of influence in any given situation. Thus Lau rejects explanations of depth which are based upon the converging and diverging functions of the eyes, because recourse to them is explaining one function only by means of another.

The first thing of importance in the configurational theories of spatial perception is that there are no elementary experiences lacking spatial properties; rather, objects emerge from a general level, much as figure and ground behave. The general spatial level is a firm structure which resists change.[48] The structure contains "anchorage points" which are the points of reference of our spatial structures. This general spatial level, or frame of reference, remains fixed in spite of eye movements, head movements and even movements of the body. We have seen how eye movements and hand movements take place in order to sharpen or modify existing spatial configurations in the di-

[48]What follows is a general configurational theory of space perception, largely on the basis of Wertheimer's theory as laid down in his 1912 paper, 253ff.

rection of better configurations, and how they may change old configurations into new configurations. But normally objects maintain their positions quite independently of the changes which the *O* makes. It is possible, with sufficiently powerful stimuli or by unusual conditions, to loosen these points of reference. Wertheimer gives an illustration which shows how a frame of reference may be destroyed.

If an after-image of the phi-phenomenon is projected on a table or chair, two possibilities are open: (1) the after-image appears to move upwards or downwards and the object on which it is projected remains stationary, in which case the ordinary frame of reference has not yet been disturbed; (2) the object on which the after-image is projected and the whole field about it seem to join in the movement of the after-image; there is movement either to the right or left, or up or down, but no stationary objects remain as fixed points of reference. The whole spatial frame of reference has now become instable and spatial perception is altered accordingly.

The configurational account thus far given does not imply that spatial perception is entirely relative, or subjective, or optional. Even though the anchorage points are loosened and ordinary configurations are so deformed or changed that new spatial structures arise, there still remain definite objective and subjective (in the sense of within-the-organism) conditions which determine the new spatial changes.

When points of anchorage are gone, says Wertheimer, localization, direction and distance judgments become erroneous. The perceptions of depth, position, apparent size and a multitude of other spatial phenomena are bound up with configurational structures. Spatial changes concern changes within the general level in which we normally find ourselves. Changes may take place within the level, which then remains firm, or they may concern the level itself. The experiments of Lewin and Sakuma show that, if a part of a configuration is moved, it may modify the configuration to which it belongs. This would necessitate a change within the level; but if the movement is carried too far it will result in double images, the possibility of accommodating the existing configuration to the change having gone, and two separate structures arise.

In close agreement with the Wertheimer theory of space are the results of von Hornbostel's experiments on the optical inversion of objects.[49] If a cube constructed out of wire is held before a mirror so that the mirror-image can be seen through the wire model, it is possible, with practice, to invert either the mirror cube or the felt cube. The result is that, if the optical cube is inverted and the tactile cube is not, the *O* finds that the cube looks like one object and feels like another! Which is the more real, the invisible but felt object, or the impalpable but seen object? Von Hornbostel's answer is that it will be the object which is made or kept convex, *i. e.*, the object which protrudes from a background. Rubin had ascribed more "thing" character to the figure than to the ground from which it protrudes,[50] so that we should expect the figure or protruding object to be the more real. The importance of these facts for the configurational theory of space consists in von Hornbostel's interpretation: the object (in this

[49]E. M. von Hornbostel, Ueber optische Inversion, *Psych. Forsch.*, 1, 1921, 130-156.

[50]E. Rubin, *op. cit.*, and Section 11, above.

case the visual object) which protrudes from a background (convexly) is an object in a definite space, whereas the felt object is not yet localized in tactile space. In merely holding it (since we are already visually-spatially minded), our tactile space-structure is not sufficiently impressive to offset the visual configuration. The tactile space becomes structured only when one moves in it, with the result that one part lifts itself from the rest,—in other words, when one establishes in touch the conditions for a figure-ground effect. If the *O* moves his fingers about the cube, it may become more real and "thingly" than the inverted mirror cube, which, in comparison with the tactile structure, has now lost its firmness. It is a matter, then, of which configuration will predominate. Often there may be rivalry between the tactile and visual spatial structures.

The inversions which he has described cannot, von Hornbostel says, be explained as illusions of perception or as the results of ideational activity on the part of the *O*. They are experienced under certain conditions which are explicable on psychological grounds, *viz.*, by changing the right-left, up-down and forward-backward anchorage or reference points of our ordinary spatial structures. Such factors as association, eye-movements and the assumption of a special psychical activity, do not explain the facts. The explanation must be in terms of the conditions which govern spatial configurations.[51]

Later configurational experimentation has borne out the original Wertheimer spatial theory. Space perception requires frames of reference in which our experiences are ordered. These frames or levels may be distorted, altered, or destroyed. Since configurations tend to preserve their form, we should expect resistance to alterations and distortions, and the appearance of new spatial structures only under special conditions. The effects of these special conditions on spatial configurations have yet to be worked out in greater detail and to be formulated in terms of general laws of space perception.

[51]V. Benussi, writing in 1911, advanced some configurational principles in explanation of reversible figures; he cannot, however, be said to have anticipated the configurationists, because in his theories associative connections still play a large part, whereas a true configurational account makes no use of associational concepts. *Cf.* Ueber die Motive der Scheinkörperlichkeit bei umkehrbaren Zeichnungen, *Arch f. d. ges. Psych.*, 20, 1911, 363-496.

THE RÔLE OF EMOTION IN A SYNAESTHETIC SUBJECT

By THOMAS D. CUTSFORTH, University of Oregon

The work of Wheeler and Cutsforth on synaesthesia has apparently tapped so many problems, not only in synaesthesia, but in general psychology as well, that it seems expedient to present certain details of another case. This case has been selected especially for the light it throws upon the content of emotion and the relationship between the so-called feeling and cognitive processes.

It will be remembered that synaesthesia as described in earlier articles[1] is unquestionably a process of perceiving. If this is true, it seemed to the writer that a study of synaesthetic processes in the field of emotion would prove enlightening; for it was suspected that the conventional distinction between feeling and cognition failed to hold in synaesthetic *Ss*. Neither the view that feelings evaluate while perception furnishes knowledge, nor the view that feeling and cognition are two aspects of the same consciousness, seemed valid in face of our preliminary investigations. Moreover, our results of more recent date brought us in touch with the problem of the stimulus error, and unquestionably threw this problem into line with our discovery of the "parent process." Meanwhile, we were still concerned with the problem of meaning.

We shall here attempt to present the facts of a particular case. It will be observed, no doubt, that our use of words presenting the reports of our *S* does not harmonize in many respects with their ordinary psychological usage. The reasons for this departure should become apparent as we proceed.

The *S* for this investigation was Miss *E*, a graduate student in the University of Oregon, a 'major' in education, but well trained in psychological procedure. Our methods in this experimentation were so varied that they will be presented in connection with results throughout the paper.

When about 9 years of age *E* became aware of her synaesthetic processes, but of course did not recognize them as such. Recognition came about

[1] R. H. Wheeler, Visual phenomena in the dreams of a blind subject, *Psychol. Rev.*, 27, 1920, 313-322; The synaesthesia of a blind subject, *Univ. of Ore. Publs.*, 1, No. 5, 1920, 61pp.; Wheeler and Cutsforth, The synaesthesia of a blind subject with comparative data from an asynaesthetic blind subject, *ibid.*, 1, No. 10, 1922, 104pp.; The rôle of synaesthesia in learning, *Jour. Exper. Psychol.*, 4, 1921, 448-468; Synaesthesia and meaning, this JOURNAL, 33, 1922, 361-384; Synaesthesia, a form of perception, *Psychol. Rev.*, 29, 1922, 212-220; Cutsforth, The rôle of synaesthesia in reasoning, this JOURNAL 35, 1924, 88-97.

through a chance discussion with a group of older girls concerning the nature of piano tones. Soon she discovered that her thinking differed markedly from that of other girls. Until she reached her senior year in college and began to act as *S* in the psychological laboratory she suffered from the misapprehension that her mental life was abnormal. Throughout this long period she made repeated efforts to avoid the use of colors in her sensory and emotional experiences; but the attempt to eradicate the visual processes was a failure, and the only result was a modification in the behavior of the imagery and a possible reduction in her mental efficiency. *E* has now been under observation for 18 months.

A typical childhood memory. Our procedure here was simple. *E* was asked to recall several of her earliest memories and then to give a complete introspective account of her mental processes. Space will not permit a publication of detailed introspective data.

In summary, *E*'s childhood experiences are recalled in terms of concrete visual and synaesthetic visual imagery. The concrete imagery appears in fragmentary form, visualized through a washed-out film of synaesthetic coloring which has been retained as the content of an emotional experience. For example, *E* recalls that when about 5 years of age she was in the middle of a fully-bloomed clover field. The red and green of the clover appear to her now in quite definite visual imagery; projected through and upon the concrete visual imagery is a gauzy film of bright rose. This, she reports, is the synaesthetic color of joy which she experienced at the time. The rose-color is not directly associated with the clover blossoms, for it is of different tint and has the full significance of an emotional experience. In fact, the rose-synaesthesia is the reduced or mechanized content of the emotion itself. In recalled, as in immediate emotions, details such as intensity, affective quality and interpreted meaning consist exclusively of synaesthetic coloration, from the standpoint of sensory content.

E's stereotyped image-forms. *E* makes constant use of schematized forms in computing numbers, in reckoning the days of the week and months of the year, also in recalling the alphabet. These forms are rich in emotional coloration, traceable to the affective color-reactions of early childhood. This coloration assumes in part the concrete hue, shape, position and behavior of early visual associations. In addition to the types of imagery just mentioned, *E* makes use of a third, which is functionally closely related to the others, but apparently derives its origin from an entirely different sense modality. Imagery of a synaesthetic nature possessing color and carrying a strong emotional meaning is described as having "perceptual tangibility" with an unmistakable reference to the body.

For example, *E*'s number form consists of a vertical row of figures from one on up indefinitely. The figures appear in pencil upon a manila-colored background. Her alphabet form con-

sists of letters visualized along a horizontal line extending from left to right. The first 7 letters are identified with the 7 notes of a tonal octave. The imagery employed in these two forms is of an entirely different concrete nature, which has been derived from a direct visual stimulus. If there was ever any emotional synaesthesis connected with these forms, it has disappeared. In view of the facts, first, that the majority of *E*'s experiences are emotionally colored in a literal sense of the term, and secondly, that she uses these two forms constantly, we have every reason to believe that the lack of emotional coloring in these two cases is evidence of a very high degree of attenuation or mechanization. It is doubtful whether further simplification will take place, for everything contained in these two forms seems necessary for their use.

On the other hand, the coloration for days of the week and months of the year includes the curious tactual factor.

Sunday is a bright golden yellow—pleasing. Monday is a dull red with an intermingling of brown. Tuesday is a bluish-grey, sky-color like diluted copper-sulphate solution. Wednesday is a "soft" shade of brown like that of high-grade chocolate. Thursday is colored similar to Tuesday, but is more highly saturated. Friday consists of shades of red, blue and yellow. The colors are opaque and pigmented. Saturday is brown, like allspice, with a strong tint of yellow.

Sunday and Friday stand out in the form as being more emotional than the other days of the week. They are more highly colored, both in degree of saturation and in brightness. In other words, they represent more emotional experience than do the other and less colorful days. Perhaps they owe their imaginal coloration to some childhood experience which determined the synaesthetic processes, or the emotional value may have been added to the original synaesthesia, subsequently. However, we are compelled to favor the former view, because of the large number of instances in which the emotion determined the nature of the synaesthetic sensation and imagery, regardless of the nature of the stimulus.

The month form is a disc which is projected in space at a convenient reading distance. The months lie about the rim of the disc at regularly spaced intervals, one color merging into its neighbors. Beginning with January the months run counter-clockwise, with the new year falling at the position of half-past four. Thus, each month occupies about 30° of the disc.

January is made up of a "tactually compressible" dusty brown splotch, unpleasant. February is an opalescent, rosy orchid, flecked with white. It is utterly intangible, like vapor, and very pleasant. March is a dusty brown like January, but less saturated. Toward the end it becomes a tan color. The month possesses an internal whirling movement, also a slight degree of tangibility; mildly unpleasant. April is made of tangible reds, blues and purples, with the clear quality of spectral colors; pleasant. May has a green background, upon which are splotched bluish purple, whitish-

pink, red, and a very light yellow. The opalescent intangibility of April has nearly vanished; pleasant. June is a continuation of May, but lighter in color, with an addition of red and blue. The month is tangible. Strong mixed emotions. July is similar to June, with the exception that it is covered with a film of grimy, dirty brown. This color as well as the concept of July possesses a reference to bodily discomfort; strongly unpleasant. August is a continuation of July, with a marked increase in the July coloration and an increase in the bodily discomfort; strongly unpleasant. September is a background of dirty brown, splashed over with yellow and green; slightly unpleasant. October is a dark grey background, partially filled with shades of orange, yellow and red. The month possesses neither tangibility nor etherial opalescence; nominally neutral. November is a background of yellow, modified by the colors of October, less saturated; pleasant. December is predominantly a dark grey with a little blue, more white, and a small amount of intense blue-black; is in a magazine picture of midnight; pleasant.

In general our *S*'s month form is similar to those possessed by other synaesthetic individuals. It functions as a concept of a year's time, when perceived as a whole, and as different seasons when perceived in divisions. The segmented parts of the disc mean individual months. The individuality of the months depends upon their characteristic color and upon their proper place on the year-dial. Two additional factors enter into this form. First, the process of perceiving a synaesthetically colored month as an emotion,—by which we mean that the emotional response is represented in the various qualities of the colored imagery. Secondly, the process of perceiving a tactual tangibility or compressibility in the synaesthetic imagery. This is not an experience of that empathic variety in which a visual image appears as if it contained cutaneous sensations. Rather, the visual imagery means a tactual experience. It is, for *E*, tactual sensation. On the other hand, opalescent and etherial colors lack this tactual increment, and the experience as a whole contains the interpretative process that tangibility is impossible. This interpretation is traceable to the attentional emphasis upon the opalescent and etherial nature of the imagery.

The tactual and emotional elements are very closely related in that one color quality is common to them both. That is, the same appearance of the color-image that means opaqueness (visual signification) and tangibility (tactual signification) also means unpleasantness. Conversely, that appearance of the color-image perceived as opalescence (visual signification) and intangibility also means or functions as pleasantness. For example, the unpleasant months of January, July and August are tactually tangible, while the pleasant months (colors) of February, April, May and December lack the positive tactual quality and appear as intangible voids filled with nothing but opalescent coloring.

It is noticeable that the colors of green and dirty brown are generally included in the images of unpleasantness. Pleasant-

ness is included in the ephemeral colors of blue, red, and yellow. The less opaque and pigmented colors tend toward a neutral state, and give evidence of the same mechanization as was mentioned in connection with the alphabet form.

Olfactory perception. In these experiments various odors were presented to the *S* by means of a small, improvised olfactometer. Perfumes, ammonia, benzoate, formalin, ether, and other stimuli were used. Immediately after the presentation of the stimulus *E* described her perception introspectively.

Her olfactory perceptions consist first of a peculiar substitution of visualized, synaesthetic and emotional responses for the usual type of synaesthesia. It is impossible for her to perceive odors in terms of their own sensory qualities or in terms of stereotyped synaesthetic equivalents. A given odor is first perceived as a visualized emotional state. Then, during the course of the emotional synaesthesis, there is a disintegration into fragmentary concrete, visual and visual synaesthetic imagery which definitely relates the experience to some similar or identical experience in the past. It is from the imagery of the latter stage that the recognition of the odor is derived. The completion of the percept varies markedly, depending upon the degree of mechanization which has taken place through the influence of frequent experiences of like odors.

Thus, the first meaning to develop in the perception of an odor is pleasantness, unpleasantness, or a mild emotion. This meaning is a context of colored imagery. The context is then elaborated and differentiated as in ordinary perceiving, although the contents remain exclusively visual.

Failure to recognize depends not only upon the absence of associations, but also upon the circumstance that concrete imagery of these experiences is associated with an emotional response. Further, a recognition may fail because of an evident inhibiting of concrete visual imagery by unusually vivid and prolonged synaesthesia of the emotional response. That is, emotional coloration preempts the entire visual field.

E's responses to olfactory stimulation passed through certain characteristic stages of mechanization. Genetically the first reaction was emotional. This consisted of a color whose behavior and attributes meant that the stimulus was an odor and that the odor was pleasant or unpleasant. It served, therefore, not only as an undifferentiated perception, but also as an affection. As familiarity with the stimulus developed, the original emotion-colors diminished in intensity and internal movement, while the response became elaborated to include concrete visual imagery having to do with the place, the time, and other circumstances under which the stimulus was presented. But this concrete imagery was not affectively indifferent until mech-

anization was complete, *i. e.*, until the original coloration was all but gone. Meanwhile these concrete associations included not only their own synaesthetic colors, but also additional coloration of their own emotional hue. As a consequence, the relatively homogeneous color of the original emotional response became a mottled, heterogeneous massing of subdued tints. Therefore, the final stage in the mechanization process, where recognition was almost immediate, was at once affective and cognitive.

Typical Introspective Data to Illustrate the Above

Stimulus: refined ammonia. As the experimenter handed me the olfactometer I cautiously put it to my nostrils, because I had been warned to go slowly. First I was aware of a showering of sparks of electric-blue and copper, localized in the upper nostrils, which seemed to emit from the membrane. This experience first meant unpleasantness, and then pain. Immediately there developed, in the surrounding regions, memory visual imagery, still consisting in part of vivid and intensive blue and copper in the form of sparks, together with concrete visual imagery of the place and of myself in the act of smelling a bottle of ammonia. In this instance the original appearance of the sparks did not identify the ammonia, but with the appearance of the concrete visual imagery the experience became meaningful in a concrete sense. I cannot get beneath the meaning; it seems to be inherent in the experience and yet there is nothing to describe as content beyond the imagery, other than an observable something attending the imagery. This something is actually present but partakes more of an inference. It is a left-over. I cannot tell what it is. It is like trying to recall something when you almost have it and cannot clear it up.

Stimulus: rose perfume. Immediately upon the presentation of the stimulus a wealth of synaesthetic imagery developed consisting of numerous shades of pink, lavender, and a suggestion of light blue. This was the content of a feeling response of intense pleasure. This imagery had barely appeared when it began to fade, give way, but seemingly to clear up again in part, in terms of concrete visual imagery of pink, white and apricot-colored roses; I saw a large bouquet of white roses localized on a table before me—the table at which I was working a short time ago—then a bush of large pink roses seen against the house. This imagery was followed by snatches of apricot-roses seen hazily in their setting as I observed them on the way to the laboratory. All of this visual imagery came in very quickly and attention then slumped. The perfume had been perceived. Again the parent process was present, especially in the first part of the experience, but I cannot get at it. I know that there was something else there aside from the emotional coloring, but at the same time it seems as if it were not there.

Perception of sound. *E*'s perception of single tones usually consists of a synaesthetic visual image which carries not only an auditory significance but also a distinct tactual meaning with some form of bodily reference. The brightness of the image is invariably dependent upon the pitch of the tone. The hue and degree of saturation of the color depend in varying degrees upon the timbre, intensity and manner of appearance of the tone, and always involve affective components. As in olfaction, therefore, synaesthetic images receive their color from

two general sources. This fact stands out all the more prominently in *E*'s responses to musical selections.

For example, the sound of a 5th octave C tuning fork is perceived in terms of visual imagery likened to a molten stream of glucose with the brightness, transparency and tactual consistency of glucose. The image is tridimensional and moves through space as long as the stimulus lasts. The two forks of 5th octave C and 4th octave G sounded together produce a single stream in which the two images behave like different currents intermingling without losing their identity. The higher note continues to be perceived as distinctly brighter, while the lower note is darker. In objective vision there occurs no analogous phenomenon to the telescoping process which *E*'s synaesthetic imagery undergoes. Beats appear as vibrations in the imagery, something like the elastic quiver of jelly. Behind all this synaesthetic imagery lurks the parent process.

It is possible to demonstrate both the effect of intensity and the effect of unpleasantness upon *E*'s auditory sensory reactions. For example, when the stimulus is steadily increased in intensity, the synaesthetic imagery of the experience will remain relatively constant up to the point at which the intensity itself produces unpleasantness. Then the synaesthetic imagery immediately takes on a different color, which carries both the meaning of unpleasantness and the meaning of an increase in the degree of intensity. Speaking in terms of the hedonic scale, the point at which the curve of affection begins to descend marks the instant at which the imagery becomes another color.

The visual synaesthesis of a mildly blown Quincke tube is described as a soft glow of brightness steadily increasing in size, fanshape, until the intensity causes the experience to become unpleasant. Then the coloring changes to a longitudinal, striated image, of poorly saturated, dirty brown. The color itself is the unpleasantness of the situation as well as the intensity of the tone, the emphasis depending upon *E*'s mental set at the time.

It would be interesting to know if the added emotion due to a rise in intensity depends upon proprioceptive stimulation. It appears as if this were the case, for, up to a certain point, the experience is a typical auditory-sensory one, characterized by the same mechanized combination of affective and primary, sensory coloration as we found in the case of olfaction. Beyond this point, added emotional coloration brings changes in meaning, and hereafter the emotional colors function both as auditory intensity signification and as emotion.

E's perception of musical selections consists of an elaborate amplification and combination of those processes which take place in the perception of single tones. A musical selection of any degree of complexity tends to become colored more and more by the emotion than by auditory factors. The whole is a blended, flowing stream of color which never ceases its movement as long as the music continues. The colors are projected out into space, where they are seen either upon or through the different external objects within *E*'s range of vision. It is diffi-

cult to describe this imagery of music, for it is changing from instant to instant, both in color and form. The change in form involves a peculiar variety of internal movement which is important in the problem of meaning and which gives mobility to the auditory consciousness.

Single instruments and solo voices produce much simpler synaesthetic patterns than do choruses and orchestral music. Below is given a series of abbreviated introspections which describe in a sketchy manner the imaginal content of short instrumental solos. The *S* was instructed to listen carefully to the music played to her on a phonograph, after which she dictated a complete introspection upon her experiences. Columbia demonstration records were used.

Violin: blues, yellows, rose shade of red; darker brown. All along molten, drawn-out mass with no break, dipping toward the lower tones and upward as the higher notes were played. Parent process.

Viola: reds, yellows, blues, more yellow and blue as the tones become higher in pitch, dipping the same as with the violin. Shades became darker as the lower notes were struck. Parent process.

Concert bass: so dark could hardly distinguish any colors. Looked like reds and greens in the twilight. Dipped slightly as in 1 and 2. In a few places, while the very lowest tones were played, the colors changed into midnight blue. Molten mass, only too thick to be stirred. Parent process.

Violin-pizzicato: fairies dressed in blue and yellow dancing on a green. The green, shadowy and somewhat colorless. The higher tones less color: these fairies had no feet or faces; were separated and distinct otherwise in form. Parent process.

String-quartet: so complicated and rapid in movement impossible to describe.

Harp: pastel shades of blue, yellow, red, and violet. They changed from one to another very rapidly, sometimes blending and sometimes contrasting; much of the time several colors were present at once; again several shades of the same color were present. The forms were rather intangible, like clouds; they changed so rapidly that there was no distinct form to any of the imagery; an ever-changing panorama of coloration. Parent process.

These introspections reveal a minimum amount of emotional coloring, for the imagery is greatly attenuated and the auditory origin predominates. The notes on violin-pizzicato illustrate a tendency on *E*'s part toward personification. Usually such a tendency consists of incomplete visual associations. In other cases, known to the experimenter, it has been observed that musical instruments produce reactions which in form and behavior appear like birds, animals and definite human individuals. Thus to one *S* the sound of a clarinet in the William Tell overture appears like an awkward wading bird of the stork-variety. Again, to another *S* the runs in Liszt's Second Hungarian Rhapsody are indefinitely personified as sober-faced, dignified old men running backward upstairs. At present we are making no attempt to explain the phenomenon or even to sug-

gest its function. However, we believe it to be an important factor in *E*'s consciousness.

Our *S*'s appreciation of music is much above the average, and is derived solely from the color-form and the behavior of her synaesthetic imagery. Three factors enter into the coloring of this imagery of musical selections. First, a mechanized color which is now dependent upon the sensory quality of the music itself; secondly, the color which is injected into the imagery by emotional states aroused by these synaesthetic experiences; and thirdly, the tactual reference which is incorporated both into the color and into the form of the visual imagery. It is difficult for *E* to analyze her appreciation of music sufficiently for us to ascertain the extent to which each one of these three factors determines the color and behavior of the imagery. In the greatly appreciated music the synaesthetic imagery never exists unmixed with emotional coloration. There occurs a point in the ascending scale of pleasantness at which the colored perception of the instruments as such changes over entirely into a synaesthetic reaction. It cannot be emphasized too strongly, however, that even here this new set of imagery functions as both perception of the music and as an emotional response to it. Usually there occurs some form of tactual reference in both the sensory and the emotional increment of the perception.

Introspective Data Illustrating the Three Different Factors

(1) Auditory factor. Stimulus: Hymn to the Sun; violin; Kreisler. The synaesthesia of the violin tones appeared as long, drawn-out, wavy festoons of red and yellow, moving over the visual field in a horizontal direction. The piano accompaniment appeared as drops of color tending toward a stone-blue. The latter were beneath the violin images in position. Small amount of emotional imagery present appearing in alterations, blendings, and obliterations of the separate piano and violin tones. (2) Emotional factor. Minuet in E; Beethoven. It was that delightfully pleasing music consisting of a full pouring-forth of color, tending toward etherial reds, yellows and greens. Occasionally these colors were richly saturated. The colors were projected out into space in the form of vapours through which I had to look in order to see objects in actual vision. This selection was perceived entirely in terms of emotional synaesthesia; the sensory factor, as such, did not appear; there was no immediate consciousness of tones as such. (3) Tactual factor. Notes from introspection taken immediately after hearing a piano selection by a friend. The piano music appeared in a synaesthetic blue with a decided tactual reference, producing an experience similar to that of running the hand over a series of velvet tubes partially inflated with air. The tactual elements represent, in meaning, the quality of music, while the blue color is the pleasing personality of the player, determining the hue of the tones.

Illustrating the same phenomena in unpleasant auditory experience. Stimulus: Jazz orchestra made up of saxophone, banjo and piano. "The experience was decidedly unpleasant; the colors were made up of turkey reds of different shades, dirty greens, and metallic sparkles of blue and copper. This unpleasantness includes a reference to the body, perceived in terms of the scraping of rough metal across the forehead, sides of the head,

around to the back of the scalp and through to the surface of the brain. This bodily reference is not tactual as such, but an interpretation from the turkey-reds and greens. The experience contains a tactual signification—the only immediate tactual experience I can describe; its content is color."

Objective vision produces an emotional synaesthetic phenomenon which corresponds very closely to that appearing in the perception of music. *E* affirms that most objective visual situations produce an accompanying color which is visualized as projected in front of, upon, or through and beyond the actual objects. These colors carry a definite emotional value of pleasantness or unpleasantness, delight or disgust. The like or dislike for objects, landscapes, *etc.*, is carried in the accompanying colors. This is best illustrated by the following quotation.

Notes on the change of coloration of a saxophone. "The boys who live in the second house from us have a jazz orchestra which always includes a saxophone. Its coloration was always a turkey red, orange, and bright blue mingled with a disagreeable shading of dirty greens and brown. Subsequently I became acquainted with the saxophone player whom I learned to like very much; he turned out to be an affable fellow with considerable personal charm. The next fall the jazz orchestra was again in full operation and I was surprised to find that the turkey reds, dirty greens and browns of the saxophone had now been modified by pastel shades with attending pleasantness. Later some boys were serenading in the neighborhood; the music did not fully awake me, but in a sort of dazed state I was conscious of tones which had the colors of a violin; this waked me still more and I listened attentively for some time, enjoying the quality of the music. I was greatly surprised to learn that it was this same boy with his saxophone. Ordinarily I should never confuse a violin with a saxophone. The confusion at this time seemed all the more unreal since I was perfectly familiar with violin and saxophone music played under similar circumstances."

If *E* happens to be looking at objects before her at the same time that she is listening to music, there frequently takes place a peculiar conflict or blending in the resulting emotional color reactions. Sometimes *E* finds herself trying to evade one conflicting emotional reaction in favor of the other. This is best illustrated in situations such as an opera or a motion-picture performance. In case the color produced by pleasantness or unpleasantness of the visual scene harmonizes with that produced in the perception of the accompanying music, the one enhances and amplifies not only the appearance of the other, but also the enjoyment and intensity of the other. When the reverse occurs, one set of colors tends to neutralize the other, both in saturation and in brightness. The same general laws of emotional coloration operate in the processes of amplification and neutralization as were found operating in the color scheme of the year form.

In summary, pleasantness as an accompaniment to vision consists of reds, blues and yellows, and as an accompaniment to audition of rose-reds, blues and brighter yellows. Unpleasant-

ness, as a visual accompaniment, consists of dirty browns and dull greens; as an auditory accompaniment it consists of dirty browns and less saturated, dead greens.

Auditory meanings in the perception of color. The content of our *S*'s perception of color has been studied very carefully with the purpose of isolating those factors which supplement the visual. Our procedure was to place a large sheet of colored paper in front of *E* with the instructions to describe minutely the color of the paper and any associated processes. Yellow, green, red, blue and green-yellow were used. Since several factors enter into *E*'s perception of objective color, it is impossible to discuss any one properly out of its relation to the entire process, although that is what seems necessary in analysis.

Upon the sensation of objective color *E* superimposes a considerable amount of synaesthetic imagery which functions as the meaningful part of the experience. This additional and supplementary imagery involves two factors: the sensory quality of the stimulus produces an emotional response which appears partly in the stimulus-color itself and partly in some additional color of synaesthetic origin, which is projected upon the objective stimulus. Incorporated within this perception of the color is the second factor, which consists of a stereotyped "association" with music. The experiencing of music, of course, is of auditory origin, and here there is no such auditory definiteness in the visual perception as there is in the tactual associations with music. It is possible, however, for *E* to judge the approximate pitch of this imaginal music from the degree of brightness in the stimulus-colors. The nature of the music derived from the stimulus-color depends upon the tint and degree of saturation. When fully described, this derived music is more like a memory of the music than the actual experience of hearing it. It is a musical experience in the absence of the parent process. *E* describes it as appearing static without internal movement within the image. Vitality is lacking in the rhythm, although the rhythmic striations appear in the imagery.

This point is illustrated by the following introspections. The Minuet, by Beethoven, was being played to the *S*. As soon as the synaesthetic coloration in her auditory perceptions was fairly well under way, the blindfold was lifted and she was confronted with a large sheet of violet paper. "The music aroused a wealth of apricot color, numerous tints of pink and lavender. Upon opening my eyes these colors were projected through and beyond the violet paper and my objective vision of the paper changed to a perception of a film-haze of violet color, which harmonized with the auditory, colored background."

Objective color, yellow: The objective color seems to fluctuate from the lighter tint to a darker tint; the latter was almost an apricot; the color as a whole produced a symphony of which the bright part of the yellow was the melody and the melody fluctuated from this to another produced by the darker, apricot tint. Neither melody was recognized as any particular selection, although both were orchestrations without indi-

vidual instruments standing out. Both melodies were above the pitch of middle C, a judgment following from the brightness of the colors. This imagery is not auditory; it is synaesthetic visual imagery with auditory signification. The signification appears in the behavior of the visual forms. Their auditory meaning is present only when the experience is perceived as a whole; but it is very definite. On analysis it resolves itself into visual imagery.

Green: The green is made up of several shades which run to the lighter and darker tints. This color produces a musical meaning more unpleasant than the above; the key is below middle C; it was perceived as the voice of a Russian singer with a symphony playing off in the distance. The green quality meant Russian and the other shades made the accompaniment.

Again, in these musical experiences that accompany objective vision we find a tendency toward personification. An account of this phenomenon can best be made in terms of a reference to olfaction. Here it was found that the sensation itself, although synaesthetic, did not function as a recognition of the odor. It was necessary to continue the process until some definite imagery appeared with which the synaesthetic, emotional color could be associated. Then recognition appeared. Likewise in the perception of the violin-pizzicato, the fairies constituted a definitization of the perception process. It appears as a growth toward differentiation, *i. e.*, an elaboration of meaning. It turns out therefore, to be an emotional experience which is made objectively meaningful in the form of a perception. The musical experience derived from objective vision is an amplification of the visual perception. Objective color has in it visual qualities which are common to the visual synaesthesia of emotions experienced in auditory perceptions. This fact probably explains the close relationship between the two types of perceiving.

Cutaneous and kinaesthetic perceptions. Bodily sensations always appear in terms of visual synaesthetic imagery. The moderate and ordinary sensations are visualized upon the body only under a special *Aufgabe* to do so. When the experience reaches a certain degree of intensity it is recognized as pain, and the synaesthesia takes on an emotional color-process projected either upon the body or out in space. When the emotional color is present it serves either as emotion or as pain.

In one of our experiments the forearm of the *S* was anaesthetized by freezing with ether. Then there was applied to the anaesthetic area a stylus which was connected with an inductorium. Kinaesthetic sensations appeared as intense and streaked grey forms with a very little yellow in them. Further up on the arm the streaks fused; thus the individual muscles and tendons lost their localization. This imagery was visualized upon the forearm.

The anaesthetic area was permitted time to become sensitive and the intensity of the electric current was increased. Upon the second application of the stylus appeared a confusion of synaesthetic colors consisting of bright red, yellow and blue. These colors were mixed pain and emotion. Part of the time they were projected upon the forearm and part of the time out in space. These pain-emotions consisted of unusually bright and "glowing" colors.

Emotions proper. Emotional states as such exist as colors projected out in the visual field. The colors last as long as the emotional stimulus lasts, and the intensity and prevalence of the color are a measure of the intensity of the emotional reaction. It is possible for *E* to carry all day the color of an emotional state, while at other times the color will immediately fade. These hues range from dark shades to light tints superimposed upon objects in the environment, to colors so saturated and vivid as to obscure the visual field.

Illustrative introspections reduced to note form. Read of accidental death of a near neighbor; the newspaper suddenly turned green and took on a swirling motion; as I read the details the color faded somewhat but for several hours my imagery took on a greenish cast. Later in the day I picked up the American Magazine and discovered a reference to an old acquaintance. The green cast changed to colors characteristic of happier moods —rose, orchid and cream-yellow.... Upon learning one day in class that we were to be quizzed on a certain assignment my imagery became flooded with a washed out green and dirty-color, with flashes of turkey-red, bright blue, bright green, and pale orange, representing a mixed emotion of fear and a feeling of helplessness.

"I was rather tired, my imagery inclined to neutral gray. The experimenter was playing Beethoven's Minuet. While the resulting imagery was normal for that selection, it was rather subdued in shade. The colorings were blues, roses, apricots, lavenders and pinks. Then the experimenter blindfolded me while the selection continued, only to find, on seeing again, a sheet of very bright magenta color before me. Instantly for me the phonograph stopped playing, *i. e.*, the magenta color obscured the colors of the tones, as a result of which they lost their emotional value but retained their meaning as mere tone in terms of indifferently colored, moving forms. Colors changed to shades of sulphur, dirty brown, *etc.* Imagery meaning great distaste. The minuet was no longer a delight; pleasant shades of color were modified by distasteful ones."

Summary and Discussion

(1) In so far as this investigation relates to the previous work of Wheeler and Cutsforth, the present results are in entire accord with those obtained heretofore. Principal among them are the following. (*a*) The process of synaesthesia is distinctly a process of perceiving or of cognizing, the associated imagery of which plays the same rôle in synaesthetic *S*s as in asynaesthetic *S*s. The only difference in the perceptions of the two groups of individuals is the stereotyped and highly colored character of the secondary increment in synaesthetic individuals. (*b*) The associated colors provide the context necessary in the

development of meaning. Constituting a part of that context is a process which the *S* is unable to describe. Any attempt to define it results in a centering of attention upon some feature of the associated image. We have chosen to give the name "parent process" to this phenomenon. It seems to be that increment which, in asynaesthetic individuals, is referred to as an olfactory, auditory, cutaneous or kinaesthetic quality. Presumably it may be called sensation as such. Its presence and absence are noted in a total-perception of any given experience; but when this noting becomes at all specific the *S* finds that he is referring to internal movent or "vitality" of the experience. Beyond this, little can be said about it at present. (*c*) The absence of this parent process is one of the identifying features of synaesthetic, imaginal experiences as opposed to "objective" experiences. (*d*) In certain individuals at least, synaesthetic phenomena pervade the entire mental life. It so happens that this statement has been true of all cases studied intensively up to the present time.

(2) Our *S* is completely synaesthetic. Not only are the perceptions in all non-visual modalities characterized by the absence of non-modal qualities as such, but visual perceptions and visual images are also in part emotional in their interpretative stages. Synaesthesis runs not only into the use of visual imagery, but also into visual imagery whose significance is emotional and tactual. That is, visual perceptions contain color-imagery of a synaesthetic character.

(3) Within the limits of this investigation all of *E*'s mental processes, other than the highly mechanized, contain an emotional increment present in terms of color-imagery. This emotional increment is present in inverse ratio to the degree of mechanization of the mental process in question. Thus, mechanization and attenuation are characterized not only by the loss of form, position, internal movement and detail, but also by desaturation of the colors having emotional value. In other words, mechanization, in *E*'s case, involves the process of approaching indifference. This situation is analogous to the diminution of meaning which results, in asynaesthetic individuals, in an approach to the sensory level of experience.

(4) The same colors, under different interpretative mental sets, function as emotion, as tactual, or as auditory perceptions.

(5) The presence of emotional increments in *E*'s perceptual processes seems to hark back to emotional reactions of early childhood common to visual, auditory and tactual stimulations; and the common denominator, or medium of association between visual, auditory and tactual meanings now operative in *E*'s mental life, is color-imagery functioning as emotion.

(6) Particularly evident, as an example of this situation, is *E*'s tendency to associate melody with objective colors. These associations are not present in terms of auditory images but in terms of an ambiguous emotional synaesthesia. That is, the emotional coloration may refer to the objective color or to the auditory meaning, equally as well, depending upon mental set.

(7) This confusion, or what would seem to the asynaesthetic *S* to be a confusion, between feeling and intellectual processes shows how closely the two modes of response are related. Indeed, it seems evident that it is impossible to differentiate between the two except upon interpretative grounds. In *E*'s case generalized or undeveloped perceptions are always as emotional as they are cognitive. In case of a full-fledged perception which passes through observable stages of definitization or elaboration, the first stage of development is a feeling process whose imagery becomes modified and elaborated as the perception becomes concrete and the object is identified.

In *E*'s case, therefore, emotion and cognition are not to be described by differences in content but by differences in attitude. The two modes of response are not to be contrasted in terms of aspects. The difference is a derived phenomenon, representing two successive stages in the course of development of a perceptual process. Neither the emotional nor the so-called cognitive stage of a perception is a matter of immediate experience; *i. e.*, they are not existential but interpretative phenomena. We are inclined to suspect that these facts are equally true of all individuals.

(8) Our results seem to throw light upon the problem of the stimulus or meaning-error. Tactual, auditory and emotional factors in *E*'s mental life, existing alone or in mixtures, are distinctly meaning phenomena, unanalysable except in terms of meaning, whose only observable content is visual. Tactual, auditory or emotional significance appears only in a total and complex experience; it is a characteristic of the complex as such, not something which can be reduced, adequately, to single contents of a sensory character. We are not here promoting the theory of imageless meanings, but we are inclined to believe that meanings are in themselves contents, deriving their structural character from synthesis. This explains why, when perceived as a whole, a certain experience possesses a definite tactual or auditory meaning; but, when analysed into its ingredients, loses its original character and becomes nothing but visual imagery. The difference between the synthetized and the analysed state is to be explained in terms of mental set. In other words, the problem is a functional one. The logical conclusion from this, of course, is the view that every experience

contains an irreducible increment—the meaning quality prior to analysis; an that no experience, however simple, exists in the absence of this functional phenomenon. In other words, as defined by conventional psychology, there is a meaning-error in every mental process. This is no other than the interpretative factor characteristic of every perception.

(9) It would seem that in *E*'s case visual perceptions involve a parent process. The redness, yellowness, greenness, *etc.*, are in reality such parent processes camouflaged, as it were, by mechanized associations of long standing, and simplified to such an extent that the awareness of color seems to be an elemental experience. But on more careful analysis this elementary character of the experience turns out to be a meaning-phenomenon and therefore complex. The ordinary *S* finds hue, saturation, brightness in his visual reactions so immediately as to lose the associative processes by means of which these colors become a phenomenon of awareness. On the other hand *E* brings such apparently extraneous associations to bear upon her objective vision as musical meanings whose contents are visual, synaesthetic images. These associations are so definite as to make possible judgments of pitch. It is a curious fact that these auditory associations are not auditory in content, for auditory processes, both sensory and imaginal, are visually synaesthetic. Hence auditory perceptions, both of direct and indirect character (by way of auditory stimulation or round-about association), are meanings whose content is visual. So far as we have been able to determine, where such auditory or emotional associations are not present, *E*'s consciousness of color is vague and indescribable.

(10) We may ask the question, why does our *S* insist upon describing so many of her experiences in terms of emotional coloration? Why can they not be described simply in terms of brightness and color qualities? Obviously for the simple and ample reason that it is impossible; for without this emotional reference the experience ceases to exist. To inhibit the emotional significance means to alter the total experience in such fashion as to make an entirely different sort of thing out of it. The same thing is true for the tactual increment in *E*'s perceptual processes. *E* is utterly helpless in attempting to describe this tactual experience in any other terms than meaning. Opalescence and etherialness are as much attributes, for her, apparently, as granularness, arealness or what not are for the conventional laboratory *S*. Attributes are seemingly indeterminate in a last analysis.

(11) Foremost in *E*'s mental life is the process of interpreting. This is functionally elemental and depends upon the circumstance that the simplest possible experience is a complex.

Introspectively it is impossible to get beneath the percept with its interpretative factor. It seems to the writer that this fact has not been taken into account sufficiently in systematic discussions.

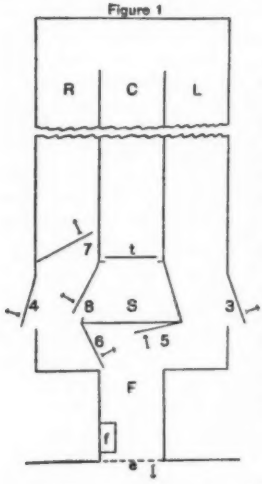
(12) The rôle of emotion in *E*'s mental life is twofold. First, it functions in undeveloped perceptions, and secondly, as the end-stage in elaborate recognition processes. Consequently, in a certain proportion of her memory processes, where mechanization has reached its limit, emotional coloration has all but disappeared. In the remainder of her recalls the emotional content, in terms of synaesthetic colors, remains in varying amounts up to complete predominance. In her case, of course, emotional experiences as such are recalled definitely in terms of visual images. We have come to the conclusion that, in a last analysis, feeling processes are undifferentiated cognitive processes, *i. e.*, perceptions in which either a labelling or identification process fails to mature, and that this fact holds for asynaesthetic individuals as well. It would seem that in ordinary laboratory investigations of feeling, *Ss* have looked for contents whose characteristics and behavior show marked differences from the so-called perceptual reactions. In the light of our results it is evident that, before we can be certain of our psychology of feeling, a great deal more careful experimentation will be necessary, in which more regard is paid to the factor of interpretation in mental life.

THE BEHAVIOR OF SHEEP AND GOATS IN LEARNING A SIMPLE MAZE

By HOWARD S. LIDDELL, Physiological Field Station, Cornell University

In the course of an investigation concerning the effect of thyroidectomy on the higher activities of the nervous system a simple maze was constructed which can be learned by sheep and goats and by means of which it is possible to study habit formation in these animals following the extirpation of the thyroid glands. The ground plan of this maze is shown in Fig. 1. A detailed description is to be found in a previous article (*1*).

Figure 1



It consists of an enclosure 67 ft. in length and 10 ft. wide covered with cinders and surrounded by a board fence. One end of the maze joins the barn, in the second story of which is the observation window covered with fine wire mesh. There are wire netting partitions between the three parallel alleys. The sheep or goat to be tested is confined in the starting compartment S. When the door t falls the animal emerges and, impelled by the gregarious instinct and usually by desire for food as well, finds its way down the alley C. Either outer alley, L or R, may be made a cul-de-sac by closing gate 5 or 6. The animal must learn to avoid the blind alley and to go up the open alley to the feeding compartment F where it can see the flock in the barn through the screen door e. Learning is judged complete when it makes three successive direct trips through the maze.

A number of cretinoid sheep and goats have already succeeded in learning the labyrinth; but in this paper only the behavior of normal sheep and goats will be considered. Three groups of sheep have learned the simple maze: first, 11 lambs from two to three weeks of age tested during May and June 1921; second, 17 May lambs tested in July and August 1922 at about two and one half months of age; and third, a group of 18 May lambs of the same year and procured from the same flock, tested in December 1922 at about six months of age.

Analysis of the behavior of the lambs in learning the simple maze. The behavior of each animal exhibited certain characteristic features which determined, to a varying extent, its pro-

ficiency in learning the labyrinth. This was noticed in testing the first pair of twin lambs. One was tentative and vacillating in its exploration of the maze. Each few steps were followed by a pause or change of direction. Its movements became slower and the pauses longer during the first trial, although it continued bleating almost constantly. The other, however, exhibited rapid and hurried movements with few and brief pauses. Its whole behavior was vigorous. Frequently it went the length of alley C at a gallop and as a result it very shortly escaped from the labyrinth in the first trial.

Another trait which influenced learning was the tendency to explore the screening between alleys. This exploration was a frequent cause of the accumulation of errors in the early trials. Standing against the screening, butting it, and trying to nose under it or jump over it often consumed much of the time required by some lambs to escape from the maze in the first few trials, while other lambs never made any persistent effort to force themselves through the screening of the alleys.

Lambs trying to escape from the labyrinth when the position of the cul-de-sac has been reversed frequently make persistent efforts to jump either the wall at the end of the blind alley or the adjacent gate closing the entrance to F, while other lambs make few or no such attempts.

Nervousness, although a definite characteristic of the behavior of certain lambs and sheep, seems to bear no direct relation to proficiency in learning the maze. It has already been noted (*1*) that as a rule the animal in F will, after two or three trials, continue eating at the food box while its pedometer is being read. (The pedometer attached to the fore leg gives a record of the animal's activity in escaping from the labyrinth.) The excitability of some lambs seems to persist. One, for example, "did not overcome her nervousness in the presence of the observer, and would not eat while the pedometer was being read, as did her twin sister, but always stood close against door e, often scratching at it before the observer appeared." In spite of this she learned both the simple labyrinth and the simple labyrinth with the position of the cul-de-sac reversed in less time and with fewer errors than her more phlegmatic sister, although at the end of the ninth trial of the first problem her nervousness was so pronounced that, in fear that she might refuse to run the maze, the tests were discontinued for the day.

One extreme case was encountered of an adult ewe which could scarcely be trained, because of her hyperexcitability, to eat in F and which never succeeded in escaping from the labyrinth. So great was her nervousness that she stood for two hours at the entrance to S and never ventured farther than halfway down alley C. There is little doubt that her unwillingness

to explore the maze was due to nervousness, since she appeared terrified and charged down C past the observer when he approached to release her from the maze. The next day she would not leave S but remained almost motionless at the entrance for two and one-half hours. It should be mentioned that the only form of punishment employed in these experiments is the presence of the observer in the maze. On a number of occasions a sheep or goat has refused to explore the maze further. If the observer then enters the labyrinth and approaches the animal it becomes terrified and makes violent efforts to escape. In subsequent trials it will usually escape from the maze with no further loitering.

This nervousness is probably only an exaggerated form of the timidity exhibited by all sheep and lambs when isolated from the flock. In certain instances, where some unusual occurrence has frightened the animal in the maze, it will refuse to run and usually can never again be induced to attempt to escape from the labyrinth. Two cases may be mentioned.

Lamb No. 1 of the 1922 series learned the simple maze in 7 trials and with but 30 errors on December 7, 1922. On December 8, with the position of the cul-de-sac reversed, the lamb, after having explored the maze for one and one half hours, was startled by the firing of a gun twice about one hundred yards distant. At the end of two hours the lamb, which had not left alley C, was driven through the maze. In a second trial, a few minutes later, the animal did not leave S and was released from the maze in 10 min. The firing of the gun may not have been the sole cause of failure, since the lamb had already been for so long in the maze, although it was observed to react to the firing—On December 13 a strong wind was blowing dead leaves about the maze. Lamb No. 6 succeeded in escaping from the labyrinth in 369 sec. with a total of 17 errors. In the second trial the animal, which appeared startled by the moving leaves, would not leave alley C and was driven through the maze at the end of 40 min. It, too, appeared to be much frightened and the leaves blowing about seemed to be the cause. Two other lambs, however, succeeded in learning the simple labyrinth the same day during the interval between the testing of Lamb No. 6 and Lamb No. 1.

A disturbing factor which was not anticipated and which affected to no small degree the results of the earlier tests was the tendency of the lambs in the maze to interrupt their explorations at frequent intervals to nibble at the few small tufts of grass and weeds in the labyrinth. They would even stop to nibble in the midst of violent efforts to escape from the maze. Finally, the tests were interrupted and the ground was covered with ashes and cinders. Even this proved ineffective in repressing the nibbling, which was recorded five times in the trial immediately following. It must be concluded that this tendency is an integral part of the behavior of the sheep in running the maze. Even though great care was taken each day to weed the labyrinth thoroughly and to remove all small objects which might attract the animal's attention, such precautions were of

no avail. At certain intervals it would begin nibbling, even nosing into the cinders and ashes. Any object, such as a small wood shaving or bit of dead leaf, served its purpose.

There is well marked difference between the behavior of the sexes in learning the labyrinth. The males are more phlegmatic in their reaction to the new situation. In general, they seem much more self-possessed. They bleat frequently, as do the females, but not so excitedly or loudly. Even the more nervous males very soon become deliberate in their exploration of the maze. In this connection reference should be made to similar observations of Yerkes (*2*) on sex differences in the discrimination behavior of the ring dove.

As yet no evidence can be presented as to the exact age at which the lamb will run the maze while its mother is confined to the barn with the rest of the flock; in other words, the age at which the incentive to learn the maze is the same as that which impels the adult sheep to learn. A lamb as young as two months will eat at the food box f at the conclusion of the tests and will learn the labyrinth without the presence of its mother in F. However, when admitted to the barn it will seek her out and suckle. A lamb of three weeks will, however, bleat continually when separated from its mother even when confined with the rest of the flock. Observations of the sheep in pasture indicate that lambs as young as two months are impelled by the gregarious instinct rather than the more specific impulse to follow the mother. At this age the lambs graze together in small groups and are often found at a considerable distance from their mothers in the pasture. When the flock is being driven to the pen for the night the ewes will bleat and the lambs reply, but they do not necessarily run to their mothers.

Although, as previously mentioned, the effect of the ewe's bleating on the young lamb in the maze indicates that in all probability the desire of the lamb to be with its mother is the incentive activating its behavior, the bleating must be classed among the disturbing factors which diminish the reliability of the learning results. In the first place, the bleating of the ewe is inconstant. When confined in F she may bleat continually, especially during the earlier trials, but if kept in the barn with the rest of the flock she bleats usually only at irregular intervals. When the older lambs and sheep are running the maze bleating among the flock in the barn seldom occurs. The lamb or sheep in the maze exhibits considerable inconstancy in bleating. Most animals bleat almost continually during the first trial in the maze and whenever a new problem is presented. Later, the occurrence of bleating is as unpredictable as in the case of the sheep confined in the barn. In the second place, the response of the lamb in the maze to the bleating of its mother

or to some other sheep or lamb in the flock cannot be predicted. It would be anticipated that whenever the lamb heard its mother bleat it would reply and run toward the source of the sound. This the young lambs usually do, but not always. They sometimes reply without moving or may even run away from the sound.

Although the older lambs and sheep, as a rule, eagerly seek the food box f after escaping from the labyrinth, it has been shown that the desire for food is not the dominant incentive. First, the animals do not always eat at the conclusion of a test. For example, in learning the alternation problem, four successive trials per day were required and the position of the cul-de-sac was reversed at every trial. Considerable difficulty was experienced by the lambs in avoiding the blind alley. It frequently happened that they became excited as the trials progressed, and in many cases they would refuse to eat during the concluding trials of each day, remaining instead near the door e through which the flock in the barn could be seen. In such cases, the lambs, which were well trained, ran the maze as usual though the incentive provided by the food was no longer operative. The same thing has been observed in the case of nervous lambs who refuse to eat between tests but nevertheless succeed in learning the maze, in certain instances more rapidly than lambs which constantly feed at the end of each trial. Secondly, it was accidentally demonstrated that when the lamb in F cannot see the flock through the door e it will not eat. In the early experiments with the labyrinth the animal was taught to go from the barn to F, not through door e, but around the barn and through gate 4. The wooden storm door at e was on one occasion left closed; so that when the lamb was driven from the barn and, entering the maze through gate 4, reached F it could not see the flock through the screening at e as usual. On every previous occasion it had begun to eat at once, but this time it did not eat and was found nervously exploring F. When the storm door was opened and the flock was again visible through the screen door e, it immediately approached the food box f and ate as usual, paying no attention to the flock in the barn. This observation has been frequently confirmed.

An interesting situation developed when one of the ewes which had been running the maze for two years dropped twin lambs during the period of learning the alternation problem. Training was at once discontinued but was resumed a short time later. Although her lambs in the barn bleated continually the ewe gave no indication of attending to them but remained quietly in the starting compartment S and walked through the maze as deliberately as usual. At the food box f, with the lambs in the barn in plain sight, bleating excitedly, she continued eat-

ing without giving them the slightest notice. This indifferent attitude toward the lambs was exhibited during the tests of the succeeding days.

In spite of the continued potency of the flock instinct, other impulses influence the sheep's behavior in the labyrinth. This was soon realized in the first series of tests when ten errorless trials were required. In the early tests each lamb, apparently, made every effort to escape from the maze. Once the path had been discovered and traversed a number of times other impulses such as curiosity and the desire to graze, attention to chance stimuli such as noises outside the maze, etc., modified its behavior. Most of the errors which appeared in the later trials were due to these interfering factors. The time records also show irregularities during the later errorless trials which are due to the tendency shown by most of the animals to traverse the maze in a leisurely fashion once they are familiar with the path and are not startled during the trials. Since the sheep, and especially the lambs, are easily distracted and are irregular in their behavior once they have traversed the maze without error, it was decided, as mentioned above, to make three errorless trials the criterion of completed learning.

A number of observations of the sheep in the maze have shown the importance of kinaesthetic impressions in determining the animal's learning.

In one case the lamb at full gallop turned toward alley L as in previous trials but turned before reaching the end of the main alley C. It therefore galloped back the way it had come. This observation has been confirmed. The lamb on these occasions always seems surprised to find itself, not at the entrance to the feeding compartment, but back at S, and its actions give every sign of bewilderment. This type of behavior forcibly suggests that the animal is depending largely on kinaesthetic rather than visual cues in running the maze. In some cases the lamb in galloping down C will, when it turns before reaching the end of the alley, run directly into the screening between the alleys. Hunter's remarks (*3*) concerning the labyrinth habits of the pigeon are interesting in this connection. He notes that Rouse, working with a maze identical with one of his own, "calls attention to the attempts of his pigeons to butt through the partitions in the first part of the maze and later to turn before the end of a given alley was reached. No such behavior was noted in the present experiments with wooden partitions. Head-on collisions with the walls never occurred."

Another example of the influence exerted on the lamb's movements by impressions from its proprioceptors is seen during the learning of the maze when the position of the cul-de-sac has been reversed. After having run the maze a number of

times it no longer enters the blind alley. Nevertheless, when it reaches the end of C, it turns toward the cul-de-sac so suddenly that it appears to be jerked in that direction. It then immediately turns and goes up the open alley. In the succeeding trials the turn toward the cul-de-sac, before entering the open alley, becomes less and less extensive; but it occurs just as suddenly and suggests just as strongly as at first that the sheep is being jerked or pulled in that direction. In one case the animal always made a sudden and complete turn, first toward the cul-de-sac, and then without pause continued the turn until it faced the open alley.

The distance receptors are, of course, not inactive during the running of the maze, but their influence is not dominant in effecting learning. Evidence has already been presented to show the influence of auditory stimuli such as bleating and loud noises. The following observation is interesting as regards the possible rôle of vision. A lamb galloping up alley L suddenly stopped and stared at the fence, strongly suggesting that the movement of its shadow had startled it. In one other case this behavior was observed. The complexity of the environment provided by the simple maze is indicated by another chance observation. The falling of the heavy trap door t eventually caused two of the boards to be broken. These were replaced by boards which were not painted gray as was the rest of the door. Two normal sheep, which had been running the maze for three years, each made a sudden pause at the entrance to the starting compartment S, after which each advanced slowly and nosed the new unpainted boards of the trap door. On succeeding trials this behavior was not repeated. However, tracks in the snow are apparently not noticed by the sheep in the maze even when freshly made in new fallen snow. For example, a sheep will leave a fresh trail leading up the open alley to the food box and will turn into the cul-de-sac where there are no tracks in the snow. The effect of change of illumination on the sheep's behavior in the maze has not been determined, although there is some evidence for believing it to be a disturbing factor.

Behavior of goats in the maze. Six normal goats and two cretins have succeeded in learning the simple maze described above. No essential differences have been observed between the sheep and goats in their method of seeking escape from the labyrinth. In Table I and II are to be found the records of the learning of the simple labyrinth and the learning of this labyrinth with the position of the cul-de-sac reversed by a young male goat and by a ram of about the same age.

TABLE I

Male goat, age 4 months

Problem: the simple labyrinth

Trial	*Errors*	*Steps*	*Time, in sec.*
1	0	1112	24
2	0	417	15
3	0	556	14

Problem: the simple labyrinth with position of cul-de-sac reversed

Trials	*Errors*	*Steps*	*Time, in sec.*
1	13	1391	989
2	33	2364	3025
3	0	417	92
4	0	278	35
5	0	417	26

TABLE II

Male sheep, age 3 months

Problem: the simple labyrinth

Trial	*Errors*	*Steps*	*Time, in sec.*
1	0	278	39
2	1	423	80
3	0	278	61
4	0	423	62
5	0	423	75

Problem: simple labyrinth with position of cul-de-sac reversed

Trial	*Errors*	*Steps*	*Time, in sec.*
1	12	1535	888
2	55	2503	3864
3	21	979	558
4	15	1113	674
5	2	423	53
6	0	423	44
7	0	423	59
8	0	556	44

Age as a factor in maze learning. Three females more than three years of age, two sheep and one goat, were tested in the simple maze and in every case refused to explore it. All were untrained in maze learning. One of these ewes was driven through the labyrinth, and in succeeding trials ran at once from S to the feeding compartment. When the position of the blind alley was then reversed, she remained at the end of the cul-de-sac until driven through the labyrinth. Again, in succeeding trials she quickly escaped by the new path with no errors. However, sheep from two and one half to three and one half years of age will readily explore the maze which they have learned as lambs after an interval as great as two years. Not only will they relearn the simple maze but they will also learn new and difficult problems such as the alternation problem.

Summary

A simple maze with a single cul-de-sac has been constructed in which the position of the cul-de-sac can be reversed. This maze has been learned by both sheep and goats.

An analysis of their behavior in escaping from this labyrinth is presented above.

There seems to be no essential difference between the maze-learning behavior of the sheep and the goat.

The adult animal will not explore the labyrinth unless it has been trained in the maze when young.

References

(1) H. S. Liddell, *Quart. Journ. Exper. Physiol.*, 13, 1923, 191.

(2) R. M. Yerkes, assisted by A. M. Eisenberg, *Journ. Animal Behav.*, 6, 1915, 41.

(3) W. S. Hunter, *Journ. Animal Behav.*, 1, 1911, 283.

THE PATELLAR TENDON REFLEX AND AFFECTIVE TONE

By H. E. Burtt, Ohio State University, and
W. W. Tuttle, University of Illinois

A technique for automatically eliciting the patellar tendon reflex has been devised by one of the writers.[1] Some incidental results in connection with other problems that were studied by this technique hinted that the reflex might be somewhat responsive to emotional conditions. Before proceeding to the specific study of such conditions it seemed worth while to make some preliminary experiments with the simple affective processes. The results of these preliminary experiments are reported in the present article.

Stimulus words were used to produce the affective states. Inasmuch as such words have been shown to produce differences in the association reaction time presumably because of affective differences[2] it seemed well to combine the KJ experiment[3] with the association reaction experiment and to record both types of reaction to each stimulus word. This procedure made possible a comparison of KJ results with those of association as influenced by affective tone.

Material. The stimulus material consisted of 120 words. These comprised the 90 used by Tolman[4] and 30 others. There were equal numbers of supposedly pleasant, unpleasant and indifferent words. The pleasant or unpleasant words belonged roughly to the following categories: sense qualities, success, failure, love, family, food, death, disgust and misfortune. They were arranged in groups of 5 words of a given category. Indifferent (I), unpleasant (U) and pleasant (P) words rotated in that sequence throughout the experiment. The complete list follows in the order of actual presentation, viz., I-1, U-1, P-1, I-2, U-2, P-2, etc.

I-1	U-1	P-1
still	rough	smooth
round	dusty	fragrant
crooked	overcast	sunny
high	dull	sparkling
narrow	dingy	silvery

[1] W. W. Tuttle, An apparatus for eliciting and recording the patellar tendon reflex, *Amer. Journ. Physiol.*, 68, 1924, 338-344.

[2] E. C. Tolman and I. Johnson, A note on association-time and feeling, this Journal, 29, 1918, 187-195.

[3] Throughout the article the symbol "KJ" will be used to denote knee jerk or patellar tendon reflex.

[4] *Op. cit.*

I-2	U-2	U-2
motion	defeat	victory
incident	failure	success
group	remorse	triumph
people	fool	genius
occupation	shame	career
I-3	**U-3**	**P-3**
business	grief	courtship
meeting	funeral	wedding
stamps	corpse	lover
student	coffin	kiss
writing	undertaker	bridesmaid
I-4	**U-4**	**P-4**
hill	snake	sister
water	sweat	mother
woods	worms	family
stones	sores	parents
road	swill	home
I-5	**U-5**	**P-5**
swinging	sour	sweet
tall	stuffy	airy
bent	dirty	clean
twisted	bitter	juicy
wide	chilly	soft
I-6	**U-6**	**P-6**
occasion	disgrace	reward
change	guilt	achievement
individual	blame	praise
action	stupidity	skill
feeling	scorn	applause
I-7	**U-7**	**P-7**
cover	poverty	banquet
soap	burglar	dinner
basket	bankrupt	peaches
gravel	blindness	candy
jacknife	illness	blossom
I-8	**U-8**	**P-8**
kettle	filthy	darling
squirrel	pimples	romance
ribbon	ugly	cupid
sentence	brutal	bridegroom
measure	snobbish	honeymoon

The words were grouped in this way in order to produce a brief cumulative effect for each category.

The remaining equipment consisted of the KJ apparatus above referred to, a Hipp chronoscope, a voice key of the Dunlap type, and an exposure apparatus of the falling shutter type, which exposed the typewritten stimulus words and made an electric contact the instant the word was completely visible.

Method. The KJ technique is described in the article referred to. In the present case it was set to elicit the reflex 6 times in the 1 min. A signal

magnet was added to the recording unit. On the disc that carried the contacts controlling the brake an additional contact was added which electromagnetically tripped the exposure apparatus. This contact was adjustable in its position on the disc, but in the present case was set to expose the stimulus 2 sec. before the hammer struck the knee. Preliminary experiments indicated that this was a suitable interval for the present purpose. An auxiliary platform was erected in front of the *S* and slightly above the arms of the chair. On this rested the exposure apparatus slightly at the *S*'s left, illuminated by a drop light, and the voice key as close as possible to the *S*'s mouth without obscuring the view of the exposure apparatus. The exposure apparatus closed the circuit through the magnets of the chronoscope and the voice key broke it. The *E* sat at a table at the *S*'s left. On the table were the chronoscope and various switches. From his seat the *E* could likewise reach the rack holding the stimulus cards. A master switch on the *E*'s table enabled him to throw in the circuit through the control contact so that just prior to the next blow on the knee the word would be exposed. The same switch set the chronoscope circuit for action.

The course of a trial was as follows. The screen was set so that the field of the exposure apparatus was covered. A small card containing the stimulus word was placed in the rack behind this screen. The *E* watched the *S* and, very shortly after one KJ, threw the master switch and started the tuning fork of the chronoscope. This latter served as a warning for the *S*. The word then appeared at the proper time followed by the reflex and the *S*'s response to the word. Meanwhile the *E* by means of a switch operating the signal magnet marked on the kymograph in a dot-dash code directly above the record of the crucial reflex the number of the trial. The master switch was then opened while the response and time were recorded and the card was changed. It will be remembered that the reflex was being elicited automatically 6 times in the 1 min. and at certain of these the stimulus word was given. The stimuli were given as rapidly as the results could be recorded, two or three per min.

Prior to the series the *S* was placed in the chair and the hammer was adjusted so as to give an average KJ, one that could be recorded conveniently on the kymograph. Untrained *S*s were run at least 5 min. prior to the stimulus words in order to familiarize them with the apparatus.

Six *S*s took part in this preliminary series. Three were graduate students and three were elementary students,—all men. Two of the former, E and F, were trained, *i. e.*, had served extensively as *S*s in other KJ experiments. The experiments were performed at Ohio State University in the Spring of 1924.

General Results

The general results of the experiment are summarized in Table I. The 6 rows give results for the 6 *S*s.

TABLE I

Average knee-jerk following Indifferent (I), Unpleasant (U) and Pleasant (P) stimulus words

S	Millimeters			Percent of I			Difference ÷ P. E.	
	I	U	P	I	U	P	I-U	I-P
A	19.6	21.7	24.8	100	111	126	0.9	2.3
B	12.2	6.5	10.9	100	53	89	4.0	0.8
C	12.0	9.4	10.6	100	78	88	3.9	2.3
D	9.6	6.3	6.5	100	66	68	2.7	2.7
E	35.2	36.1	35.2	100	103	100	0.3	0.03
F	33.1	30.3	31.8	100	91	96	1.7	0.8
Av.				100	84	94		

The first three columns of figures give the average height in mm. of all the KJs to stimulus words of a given sort, indifferent, unpleasant and pleasant. These mm.-figures, however, do not represent the actual height of the kick, because the amplitude was geared down in order to fit the kymograph. Some of the *S*s obviously have a much greater kick in these results than do others. The next three columns of the Table are designed to make the results of the different *S*s comparable. The crucial point is the extent to which affective tone increases or decreases the KJ relative to conditions with no affective tone. The figures for U and P for each *S* are divided by the figure for I. These ratios appear in the middle block of the Table. The differences between U and I and between P and I may then be noted. In order to determine whether such differences are significant, their probable errors are computed. The ratios of difference to the probable errors of difference appear in the last two columns of the Table. For instance, *S* A had an average kick of 19.6 mm. on indifferent stimuli, of 21.7 on unpleasant stimuli, and of 24.8 on pleasant. Reducing these figures to ratios, the results for U are 111% of those for I, and those for P are 126% of those for I. The difference between U and I is 0.9 the probable error of difference, and that between I and P 2.3 the probable error of difference. The records for all the *S*s do not include the entire 120 words. Some did not have time to finish the entire series. The discrepancy is, of course, taken care of in the probable error.

The most striking thing in the Table is the apparent depression of the KJ by unpleasant stimulus words. The 6 *S*s on U average 84% as much as on I. Four of the *S*s show differences of this sort and two the reverse, but the latter differences are small, and only 0.3 and 0.9 times the probable error of difference. The other four differences are more significant, especially those for *S*s B, C and D. These, by the way, are untrained *S*s. It may be possible that the reflex of the trained *S*s is less susceptible to the affective state, or it may be that such words aroused less affective tone in these *S*s.

With the pleasant words the results are not as clear. There is some slight evidence of decrease in the reflex. Four *S*s show slight differences of this sort. One *S* shows a considerable difference in the other direction.

It was noted that the general level of the KJ as indicated by the intermediate reactions on which word stimuli were not given (and which are not included in the Table) varied somewhat during the experiment. It is possible that the low or high record for a particular group of words may be due to its position in the series rather than to its intrinsic quality. In order to check this possibility, curves are plotted (see Fig. 1) for the three

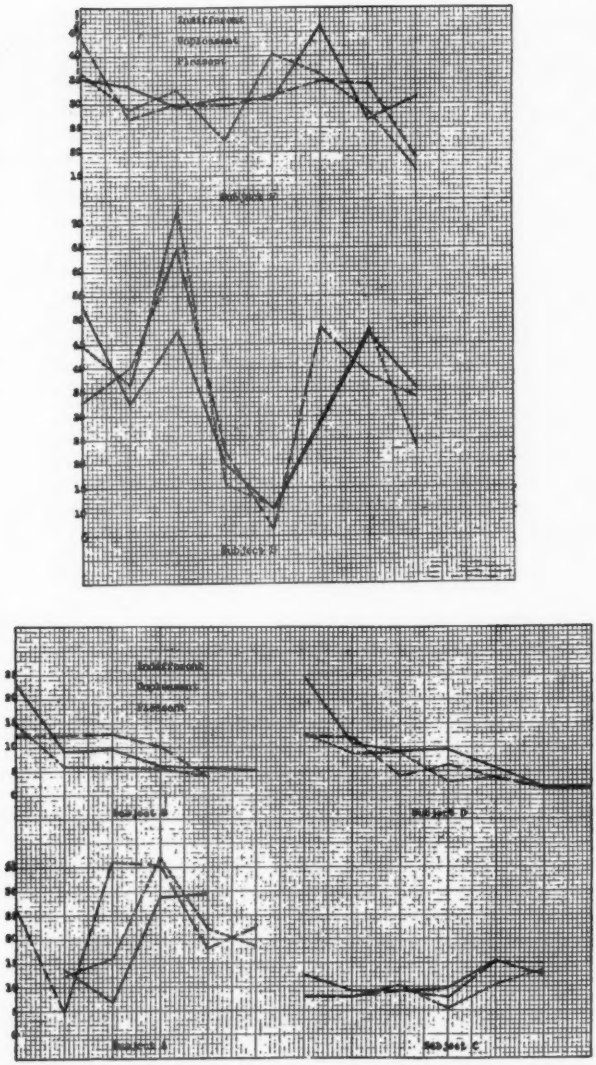


FIG. I

kinds of words, I, U and P, with the abscissa representing position in the series and the ordinate the average KJ for the 5 successive words. For the I curve that first ordinate is the average KJ for the 5 words denoted in the previous discussion of material as I-1, the second ordinate for this curve is the average of I-2, etc. Similarly, for the U curve, the first ordinate is the average KJ for the 5 words denoted U-1, the second ordinate the average of U-2, etc. The three successive groups I-1, U-1 and P-1 are plotted on the same ordinate; I-2, U-2 and P-2 are likewise plotted on the same ordinate. The three curves plotted in this way make possible the comparison of U or P at any point with the I that had very shortly preceded, and might be taken as an index of the normal KJ at that particular position in the series. In a few instances there is a break in some curve due to incomplete data. In these instances the available points on the curve are connected to bridge the gap. The solid line represents indifferent words, the broken line pleasant, and the dotted line unpleasant words.

The results of Fig. 1 tend to corroborate those of the preceding Table. With *Ss* B, C and D the curve for U is rather consistently below that for I. F shows the same tendency to a lesser degree, E is equivocal, and A is rather consistently in the opposite direction. The curves for P are inconclusive. With D the P curve is rather consistently below the I curve, with A the reverse is true; but the others are rather equivocal. It would seem that the results shown in Table I are due to the actual effect of the stimulus words rather than to the position of the words in the series.

A further problem arises as to whether the differences shown are due to certain types of words more than others or merely represent the average effect. Table II is designed to bring out this point and the KJs are classified according to the nature of the stimulus words.

TABLE II

	Ratio of Unpleasant to Indifferent					Ratio of Pleasant to Indifferent				
S	Sense	Failure	Death	Disgust	Misfortune	Sense	Success	Love	Family	Food
A	76	90	236	130		62	35	501	124	
B	65		60	90		76	137	133	165	
C	67*	88*	111	56		82*	92*	98	81	
D	57*	90*	96	29	80	56*	100*	43	67	80
E	95*	108	153	72*	102	61*	119	96	113	82
F	116*	81*	111	61*	107	113*	78*	80*	95	128
Avg.	79	91	128	73	96	75	93	168	107	96

The ratio of each 5 unpleasant words of a given category is taken to the 5 indifferent words preceding it. This is equivalent to taking the two values from a given ordinate in the curves of Fig. 1 and finding the ratio of one to the other. In cases where

two groups of five words of the same category are available, the ratios for the two are averaged. Such cases are starred in the Table. Exactly the same procedure is followed with the pleasant stimulus words. For instance with *S* A, the ratio of the average KJ on the 5 unpleasant words dealing with sense qualities to the preceding 5 indifferent words is 76; the ratio of the average KJ for the 5 words dealing with failure to the preceding indifferent words is 90, etc.

With unpleasant words, the most pronounced decreases on the average are shown for sense qualities and disgust, with minor decreases for failure and misfortune and an increase for death. However, B shows his greatest decrease of all with death. Four other *S*s show their greatest decrease with disgust. We may conclude that in our results all the categories are in some instances effective in depressing the reflex, and that perhaps disgust is a trifle more effective in general than the other words. Whether this is due to the category or to the fact that these particular words provoked a more marked affective tone cannot be determined. The *S*s were questioned after the experiment as to the points that impressed them the most and whether they found some words more pleasant or unpleasant than others, but little of interest was forthcoming in their statements. None of the *S*s had had any training in introspection and instructions prior to the experiment to attend to affective tone were avoided because the effect of attention might introduce another variable and complicate the results.[5] The words with which the *S*s reacted indicated in most cases the normal association that might be expected. The stimulus words practically always called up something in the category intended by the *E*.

We may conclude, then, that unpleasant stimulus words in general have a slight but not universal tendency to decrease the patellar reflex.

With the pleasant words the case is not as clear. The most uniform category seems to be sense qualities, with 5 of the 6 *S*s showing decreases that average 25%. Love shows a considerable average increase, due, however, to the extraordinary results of one *S*. Two *S*s show their greatest decrease for success, two for sense qualities, one for love, and one for family. It appears that no one category is outstandingly potent in producing depression, but all are perhaps slightly effective in that way.

The association reaction time was recorded for each word except in a few instances where the chronoscope failed to work. The times for all the indifferent words for a given *S* were aver-

[5] W. W. Tuttle, The effect of attention or mental activity on the patellar tendon reflex, *Journ. Exper. Psychol.*, 7, 1924, 401-419.

aged, and likewise for unpleasant and pleasant. These averages are given in Table III.

The averages are in σ and the small number following each average is the number of cases. All the *S*s agree in a longer

TABLE III

	Average Association Reaction Time						Difference ÷ P. E.
S	I		U		P		I-U
A	659	28	824	25	733	29	3.3
B	558	25	576	21	548	16	0.9
C	877	28	1026	25	814	23	2.5
D	654	28	783	31	666	26	2.3
E	848	37	978	32	833	35	2.3
F	516	30	614	34	576	36	2.5
Avg.	685		800		695		

average reaction time for unpleasant stimuli. Three are quicker on the pleasant than on the indifferent, and three the reverse; and the averages of all *S*s for I and P are almost identical. The differences between I and U are evaluated from the standpoint of probable error of difference in the last column of the Table. While the ratio of difference to its probable error is small for most *S*s, the fact that all the *S*s are slower on U than on I deserves some consideration.

The finding corroborates the results of Tolman, who found that "the pleasant words as stimuli were not noticeably different from the indifferent words; but the unpleasant words showed a decided tendency to cause longer association times".[6]

We find, then, evidence of decrease in KJ and increase in association reaction time for unpleasant stimulus words. This correspondence suggests that the change in the KJ is due to the same factor as the change in the reaction time. To study this relation further the two variables were correlated, *i. e.*, a scatter plot was made for each *S*, plotting for each word the association time against the KJ. These correlations are given in Table IV.

TABLE IV

Correlation between KJ and Association Time

S	r	P. E.
A	—.11	.08
B	.08	.09
C	—.14	.07
D	—.29	.08
E	.08	.07
F	—.18	.07

[6] *Op. cit.*, 189.

The correlations are not large, but four of them are negative. The two positive ones are manifestly insignificant, and so is one of the negative. This is just a slight substantiation of the point that KJ and reaction changes are due to the same factor. Inasmuch as it has been elsewhere shown that the latter are due to affective tone of the stimulus words, it is probable that the KJ results are likewise due to this same affective factor. The conclusion is probably warranted that unpleasant stimulus words, those that appeal to simple sense qualities as well as those that deal with failure, disgust, death and misfortune, serve to produce a small amount of depression in the patellar tendon reflex.

Summary

A technique was available for eliciting the patellar tendon reflex periodically. Stimulus words—pleasant, unpleasant and indifferent—were presented visually just prior to certain elicitations of the reflex. The extent of the reflex and the association reaction time were noted.

There is a depression of the reflex on the average for unpleasant stimulus words amounting to 16%. There are slight and less consistent indications of a similar depression for pleasant stimulus words. All the unpleasant categories used were effective in some instances in producing depression.

The average association time for unpleasant stimuli is slower than for indifferent stimuli with all *S*s. The correlations between the extent of a reflex and the association reaction time for the stimulus word presented in connection with it are small, but all those that are of possible statistical significance are negative. This result suggests that the depression of the reflex and the slowing of the association time involve the same aspect of the stimuli, presumably the affective tone.

CONSTANCY OF ATTITUDE IN WEIGHT PERCEPTION

By Earl S. Rudisill, University of Pennsylvania

For some time the difference between process attitude and stimulus attitude in the judging of sensory qualities has been recognized. Titchener[1] made this distinction theoretically and others have subsequently taken cognizance of it.

The work of deLaski[2] and Gates[3] on dual impression on the skin indicated that such differences could be demonstrated experimentally.

Friedländer[4] made the assumption of two possible attitudes in weight discrimination, process attitude and stimulus attitude. Within the former he further differentiated experimentally pressure upon the fingers and kinaesthesis in the wrist. With these differences in mind a number of experiments were performed. But Friedländer's instructions were too indefinite and equivocal, and his statistical results were so equivocal as to be incomprehensible. The chief value of his work, so far as the present problem is concerned, lies in the fact that he has shown the possibility of adopting either process or stimulus attitude under direction of attention and of instruction.

Fernberger[5] carried out an investigation of the effects of process attitude and stimulus attitude in the judging of lifted weights. He had previously demonstrated the exceedingly complex character of such judgments.[6] "It has been found that pressure sensations on the tips of the fingers, kinaesthetic sensations localized in the hand, wrist and forearm might all be involved. In the early stages of practice the pressure criteria seem to be most used but, after considerable progressive practice, most *Os* tend to form their judgments on the basis of kinaesthetic criteria. . . Here, then, would be an excellent field in which to discover whether the judging under the stimulus attitude or the judging of some particular attributive process will have any effect upon the statistical limens."[7] Fernberger used three *Os* each of whom worked under each of three instructions. The first, instruction called for judgments with pressure on the finger-tips as the criterion; the second on the basis of kinaesthetic sensations in the wrist; and the third on the basis of the weights themselves (stimulus attitude). 1250 judgments were given by each *O* under each instruction. An introspection was taken for each 250 judgments. This study yielded quite as equivocal statistical results as that of Friedländer. The reports of the *Os* indicated that the instructions had been followed.

---

[1]E. B. Titchener, *Experimental Psychology*, ii, 1905.

[2]E. de Laski, On Perceptive Forms below the Level of the Two-Point Limen, this Journal, 27, 1916, 569-571.

[3]E. J. Gates, The Determination of the Limens of Single and Dual Impressions by the Method of Constant Stimuli, *ibid.*, 26, 1915, 152-157.

[4]H. F. Friedländer, Die Wahrnehmung der Schwere, *Zts. f. Psych.*, 83, 1920, 129-210.

[5]S. W. Fernberger, An Experimental Study of the 'Stimulus Error,' *Jour. Exp. Psychol.*, 4, 1921, 63-76.

[6]S. W. Fernberger, An Introspective Analysis of the Process of Comparing, *Psych. Mon.*, 26, 1919, 161.

[7]S. W. Fernberger, An Experimental Study of the 'Stimulus Error', *Jour. Exp. Psych.*, 4, 1921, 64-65.

Reid[8], regarding the previous studies as inconclusive, carried out a lengthy investigation. He has reviewed the work of the two previous investigators and criticized both studies. Friedländer's work is first noticed. "We cannot consider Friedländer's work satisfactory as regards either his conditions, his statistical data, or his attempts at interpretation. He operates with different numbers of variables under the two *Einstellungen*, presents some variables less often than others, employs only one time order, alternates instructions from day to day, allows *O* to judge in terms of either weight, has but two *O*s, and finally, obtains only a small number of judgments (41) for every variable. Secondly, he publishes the data of only one *O*, and combines the judgments of different categories and for different variables. Finally, he offers the same explanation (the difficulty of the G-*Einstellung*) now for the result A>G and now for the result G>A,—even in the case of one and the same *O*." Reid then considers Fernberger's investigation. The latter's statement—"All of the earlier work in this field has been performed under the stimulus attitude — the *O*s have been instructed to judge the difference between the weights themselves"—is called in question, since Friedländer has used the process attitude. The validity of this criticism hinges upon the point at which one draws a line of demarcation between earlier and later work. Fernberger undoubtedly had in mind as earlier work that of Fechner and Urban, as well as his own earlier researches. The rate of lift in Fernberger's experiment is misstated as 72 strokes of the metronome per minute instead of 92. "Fernberger...works with only one time order; he does not keep the interval between double lifts constant; and he has but three *O*s. These, however, may be relatively unimportant matters; it is of far greater importance that Fernberger has not given any detailed discussion of the carriage of his instructions."[9] We may note in regard to the last criticism that while Fernberger does not print the actual introspections he does report the substance of them.

The assumption that instructions for the process attitude were actually fulfilled is questioned. "We remark...that in the case of so complex a perception as that of lifted weights the change from stimulus instruction to process instruction does not at all guarantee a change from commonsense attitude to psychological attitude on the part of the *O*. A highly trained and reliable *O* will, of course, do his best, with full awareness of what he is about, to effect the prescribed change of attitude; but the untrained *O* may very well shift his attention from weight to pressure pattern, as from one object to another, without any corresponding change of total attitude. It is therefore, altogether possible that the changes of instruction carried generically different meanings to Fernberger's trained *O* and to his other two *O*s."[10] It is further asserted that "verbal understanding of an instruction, on the part of the untrained *O*s, does not necessarily imply its assimilation and its realization in practice,—still less the steady maintenance of the correlated attitude."[11] This would undoubtedly be the case with an *O* who had undergone no psychological training; but no such *O* took part in the experiment. It is also partially true of an *O* who is unpractised in this particular kind of experimental work. But with progressive practice the condition is changed.

Reid[12], in his experimental investigation, used seven *O*s of different degrees of training, six of whom were graduate students and one an assis-

---

[8] A. C. Reid, The Effect of Varied Instructions on the Perception of Lifted Weights, this JOURNAL, 35, 1924, 53-74.

[9] *Op. cit.*, 58.

[10] *Ibid.*, 58.

[11] *Ibid.*, 58.

[12] *Ibid.*, 59ff.

tant professor of psychology. He took 2500 observations under each instruction for each *O*, and 1000 as control under the initial instruction for the particular *O*. A characterization of the criteria of judgments was taken from each *O* at the close of each series. The space error was eliminated by means of a carrier bracket[13]. Titchener's modified form of Fechner's weights was employed. The standard weight was 700 gm., while the comparison weights ranged from 649 to 751 gm., in intervals of 25.5 gm. The *O*s lifted the weights with palm downward. The time of lifting was regulated by a metronome set to 60 beats per min., with the bell sounding on every fourth stroke. Thus the interval between double lifts was 4 sec. In order to render the performance automatic, a preliminary series of about 500 judgments was given. Reid employed three sets of instructions which definitely called for attitudes leading to judgments of the weights as material objects, of pressure patterns on the thumb and fingers, and of kinaesthetic patterns in the wrist.

Reid ranked the several series in descending order of excellence (on the basis of limens and precision) for each *O*. For convenience the relations are put in the form of mathematical formulas. To the left of this formula he indicates the expected practice effect according to the order of the series for each *O*, while to the right he presents a third formula expressing (in descending order) excellence in carriage of instructions, as indicated by the reports given by the *O*s upon the completion of each series.

As to progressive practice he discovered evidence for intra-serial effect in the case of one *O* in all three attitudes; for two other *O*s in the stimulus attitude series; and for one *O* in finger-tip pressure and stimulus attitude series. The series mentioned are only those in which the *O*s concerned worked with uncontaminated attitude. The assumption that contamination neutralizes or prevents practice effect is violated in the case of subject M in two uncontaminated series. Practice effect in homologous series is found for three *O*s. In the case of one *O* practice effect through heterogeneous series was found.

A study of tendency of judgment and time order shows that 6 out of 7 *O*s gave more correct judgments when the first time order was used, that is, when the standard stimulus was presented first. More 'greater' than 'lighter' judgments were given by each *O* for every series, except in the control series for one *O*. All *O*s but one gave more correct judgments when a heavier weight followed a lighter one.

Although much critical emphasis is placed on the disregarding of the doubtful judgments by earlier investigators, Reid was not able to find that anything correlated with them, except that they grouped themselves about the smallest differences.

The final statement of this author is to the effect that stimulus instruction yielded the most accurate results. Finger-tip pressure was next and the wrist patterns ranked third in precision.

Reid worked with two time orders and thus accomplished an approximate elimination of the time error, or at least a counter-balancing of the time errors in opposite directions. But while this questionable advantage was gained there was undoubtedly a tendency, due to the attendant opposition process, to break down or prevent the establishment of a stable image of the standard stimulus. There is, we believe, a distinct advantage in the direction of stabilization of judgments connected with the use of the first time order alone, that is, standard stimulus first. A single time order helps render automatic all the factors which are common to all three attitudes in lifting and judging. This condition furthers precision, for the *O* is thus more nearly free to concentrate attention on the attributive phase of process or upon stimulus as may be required. This contention is borne out by

[13]E. B. Titchener, *Experimental Psychology*, ii, I, 1905, 116.

the fact that Reid secured more correct judgments under the first time order than under the second.

The reports which the *Os* were required to give upon the carriage of instructions were made upon the conclusion of a complete series under a given instruction. Thus they were reporting on fulfillment of instructions as long as a month after the actual experiment. Such reports can be regarded as little more than impressionistic reports on the *Einstellung* except for the work completed immediately before the dictation of these characterizations. It may be reasonable to suspect that what really happened was that, with the fading of memory as to the earlier fulfillment of instructions, the type of fulfillment which was experienced in the very last portion was ascribed to the entire series. While there is a fair correlation between fulfillment of instructions (according to the reports) and the effect of progressive practice, as well as with precision of judgment, for the reasons given above, the reports are not to be relied upon, it might seem, as truly indicative of contamination or purity of attitude.

In the light of these criticisms, it seemed worth while to repeat this experiment in order, by a more frequent instrospective check, better to determine constancy of attitude and also to check the equivocality of the statistical results of all the previous investigators.

The purpose of the present investigation, then, is further to study the possibility of adopting different attitudes in the judging of weight, to determine the statistical effects of varied instructions, to study introspectively the comparison consciousness as employed in differential weight perceptions, and primarily to investigate the possible constancy of attitude under such an experimental situation.

Procedure

We employed four *Os*[14]: M. K. MacDonald, M. A. (M), instructor in psychology; H. S. Oberly, Ph. D. (O), instructor in psychology; S. Sanderson, M. A. (S), instructor in psychology; and L. D. Stratton, Ph. D. (St), graduate student in psychology. O, S, and St were in about the same stage of practice, that described by Fernberger as the transition period between earlier and later practice.[15]. M was in a later stage of practice.

Hard rubber weights were used as stimuli.[16] The standard weight was 100 gm. It was presented with 7 comparison weights of 84, 88, 92, 96, 100, 104 and 108 gm. The greatest variation shown by any of these weights during the experimentation was but .2 gm, while the average variation was .16 gm. The deviation in the case of each weight was negative. The weights were arranged in a double series on a turning-top table. The order was such as to obviate any effect of sequence.[17] The *O* sat in a chair fitted with a padded arm. An upright screen was erected, with an aperture through which *O*'s right arm extended, palm downward. His hand was placed in position conveniently to lift the weights as the table was turned. The space error was eliminated in this way. The rate of lift was regulated

[14]I am greatly obliged to Professor Fernberger for the suggestion of this problem, as well as for his valued counsel and assistance in the preparation of this paper. Grateful acknowledgement is also due to the *Os* who made possible this study.

[15]S. W. Fernberger, An Experimental Study of the 'Stimulus Error', *Jour. Exp. Psychol.*, 4, 1921, 74-75.

[16]S. W. Fernberger, A New Form of Stimuli for Lifted Weight Experiments, this JOURNAL, 31, 1920, 147-151.

[17]S. W. Fernberger, Interdependence of Judgments within the Series for the Method of Constant Stimuli, *Jour. Exp. Psychol.*, 3, 1920, 126-150.

by means of a metronome which was set at 92 strokes per min. The bell sounded at every fourth beat. A complete lift was accomplished in the time of 4 strokes. *E* sat at the table about 100° to the right of *O* and turned the table on every fourth beat of the metronome. In this manner the standard and the comparison weights were in turn placed directly under *O*'s palm. The process was continuous except that *O* was allowed a rest whenever he so indicated. Rests were taken from one to four times in a period of time required for 112 or 126 judgments. Judgments were taken in groups of 350, that is, 50 on each pair of weights. At the end of the 112th, 224th and 350th judgment introspections as to the criteria of judgment were written by *O*. We, therefore, have 18 introspective reports from each *O* under each instruction. The points at which introspections were taken divided each group of judgments into three parts, the end of any one of which was a convenient stopping place for any day's work. We shall hereinafter refer to 350 successive judgments as a *group*, the tripartite sections of a group as *divisions*, and all of the judgments under a specified instruction as a *series*. Either one or two divisions could be done at a sitting, that is 112 (or 126) or 224 (or 238) judgments. This was left to the discretion of the *O* and was decided on the basis of fitness for further work. Not more than 350 judgments in succession were taken under any one instruction. A similar number were than taken in another attitude, and so on. Each *O* had a preliminary series. A 'warming up' practice of at least 14 judgments preceded each period of work.

Each *O* made 2100 judgments under each of three instructions. The following instructions were given to the *O*s in the first series (A).

"Certain stimuli will be presented to you which will be lifted— in so far as the rhythm and manner of lifting is concerned— in accordance with your former instructions.

"You will report only on the intensity of the pressure sensations on the tips of the fingers. Attend to these pressure sensations only. Three categories of judgment are permitted—'greater', 'less', and 'equal'. A greater or a less judgment indicates that the intensity of the pressure sensations on the tips of the fingers aroused by the second stimulus was greater or less respectively than the intensity of the pressure sensations aroused by the first stimulus of the same pair. An equal judgemnt indicates that the intensities of the pressure sensations aroused by the two members of a pair were subjectively equal.

"If you fail to live up to these instructions, either with regard to the categories of judgment or with regard to the formation of the judgment on the basis of the intensity of the pressure sensations only, you will not give a judgment but will indicate the fact by reporting the word 'failure.'

"Intensity of pressure sensations in the above is to be taken the sense of mental process only."

The second series of judgments (B) was made under instructions which required judgment on the basis of the kinaesthetic sensations in the wrist. The instructions were identical with those for series A except that the words "intensity of kinaesthetic sensations localized in the wrist" were substituted for the words "intensity of pressure sensations on the tips of the fingers."

The instructions for series C were framed with the intention of inducing the stimulus attitude. They were as follows.

"Certain stimuli will be presented to you which will be lifted—in so far as the rhythim and the manner of lifting is concerned— in accordance with your former instructions.

"You are to take the stimulus attitude and are to judge the weights themselves. Three categories of judgment are permitted—'greater', 'less', and 'equal.' A greater or a less judgment indicates that the second weight was greater or less respectively than the first weight of the same pair. An

equal judgment indicates that the two stimuli of a pair were of equal weight.

"If you fail to live up to these instructions for any reason you will not give a judgment but will indicate the fact by reporting the word 'failure.' "

Following this work an introspective study of the process involved in comparing the members of the pairs of weights was carried out under each of the three instructions. After a preliminary practice at each experimental sitting the same instructions, which were previously used, were read to the *O*. He was asked to make judgment only on two pairs of weights successively and then to write an introspective report on the mental processes involved in making judgment on the second pair. An introspection or report was given on each of the 14 pairs by each *O* under each instruction. The conditions of lifting were the same as in the previous work.

Results

A record of all judgments, along with their distribution, is given in Tables I-IV. The observed frequencies of judgment for each *S* are found each in a table. All tables have a similar distribution. In the first columns are found the letters representing the different attitudes under which the results were obtained. The remaining columns are grouped in threes and give successively the frequencies of lighter, equal and heavier judgments for each of the seven comparison pairs. The frequencies for each group are found in a single row. In the three rows at the bottom of the table will be found the totals of frequency of judgment under each of the three attitudes.

In Tables V-VIII are presented the h-values, the limens (S_1 and S_2), the intervals of uncertainty, and the points of subjective equality for each *O* under each instruction. Table IX is an exhibit of the tendency of correct judgments and of the tendency of all judgments. In Table X we have recorded the relation between the qualitative and the quantitative results. The first column indicates relative fidelity to instructions (purity of attitude) in the several series. Highest excellence is indicated by the numeral 1 and lowest by 3. The letters refer to the several series and the work under them. B opposite 1 in the 'Purity of Attitude' column indicates that for the *O* designated on the margin the kinaesthetic instructions were best fulfilled. C opposite 1 in the 'h' column indicates that for the *S* concerned the greatest accuracy was achieved under the stimulus attitude. Under practice effect we show the relative effect of progressive practice under the several instructions, the greatest amount of such effect being set opposite 1. Table XI records the indices of significance of difference computed according to Boring's formula.[18]

Discussion: A. General Ranking

The relative standing of the several *O*s may be determined by reference to Tables V-IX. M ranks third in precision of

[18] E. G. Boring, this JOURNAL, 27, 1916, 315ff; 28, 1917, 454ff.

judgment, with an average *h* of 0.086 and a range extending from 0.054 to 0.118. He is third also in smallness of the intervals of uncertainty (I. U.) with an average of 6.33 and a range from 2.65 to 11.17; this value is indicative of the degree of sensitivity. As to magnitude of the point of subjective equality (P. S. E.) he holds second place; the point is 95.32 and the variation ranges from 94.17 to 96.55. This indicates that only one other *O* was less influenced by the time error. As to the number of correct judgments M stands third with 3586 out of a total of 6300. He gave the smallest number of 'greater' judgments and the second largest number of 'less' and 'equal' decisions.

O has the second largest average *h*, 0.0961. The spread of these values extends from 0.079 to 0.135. He holds the same rank as to the I. U.'s with an average of 5.50; the figures vary from 4.39 to 6.76. In regard to the P. S. E.'s O's position is third. The average is 94.88 and the spread is from 93.96 to 96.44. He gave the second largest number of correct judgments, namely 3704; he occupies the same rank for total number of 'greater' judgments, while he stands third in number of 'less' and 'equal' decisions.

As to accuracy in comparison decisions S is in fourth position. His average *h* is 0.0753 while the variation extends from 0.040 to 0.132. As respects the I. U.'s he ranks fourth also; his average is 7.31 and the range is from 2.04 to 13.42. His mean P. S. E. places him in the same position; the extremes are 90.95 and 97.61. S gave the lowest number of correct judgments. He ranks third in number of 'greater' judgments with 2587, fourth on the 'less' judgments with 2248, and first on 'equal' comparisons with 1465.

St holds first place in every respect except in number of equal judgments. His average *h* is 0.1095 and the spread is from 0.089 to 0.153. The mean I. U. is 1.27 with a deviation from 92.68 to 97.42. He gave 4515 correct judgments, 3096 'greater,' 2957 'less' and 247 'equal' comparisons. He is in fourth place for equality judgments. While this measure is fourth as to number of comparison decisions it indicates highest sensitivity.

B. Purity of Attitude

In this section we present a detailed account of the carriage of instructions as revealed by the 18 introspections or reports given by each *O* under each instruction. A sampling of three characteristic reports taken at varying stages of the work accompanies the account. The numbers attached to the sample reports refer to divisions.

M ranks second among the *O*s as to purity of attitude. Under instruction A he judged in terms of the size of the area

stimulated on the finger-tips. This criterion remained constant throughout the experimentation after some initial practice. It is specifically mentioned in 15 out of 18 introspections. In the first and second divisions there was difficulty in ruling out the kinaesthesis in the forearm and in the fifth division stimulus attitude interfered to some extent. There was marked visual imagery of the arm, the hand, the fingers and the weights throughout. Low to fair assurance as to the correctness of judgments prevailed.

Observer M: Attitude A

2. "The method of judgment remains the same, *i. e.*, in terms of the size of the area stimulated on the finger-tips. If much of the finger-tip must touch the weight, then it is greater; if it may be lifted by simple contact of the fingers, judgment is less. I find it particularly difficult to keep a constant attitude and rule out kinaesthesis in the forearm muscles."

13. "Judgment is in terms of the area stimulated on the finger-tips as previously described under this instruction. The visual imagery is shifted forward from the wrist and forearm muscles to the fingers and hand and the weight itself. But the finger-tips stand out especially."

17. "Judgment is still in terms of amount of areal pressure on the finger-tips. The visual imagery of the fingers is marked and constantly present."

While working according to instruction B, M made his comparisons on the basis of kinaesthetic sensations in the wrist. Every one of the introspections cites this basis. In the second division weight and pressure attitudes interfered somewhat. Fatigue came on rapidly. There was visual imagery of wrist and forearm, especially in the latter half of the work. Clearness of kinaesthesis varied from sitting to sitting.

Attitude B

1. "Judgment in terms of kinaesthetic sensations of the wrist not so difficult as I had thought it would be when instructions were first read. I interpret wrist liberally as the cues seem to come largely from a muscle on the volar side of the forearm. Its contraction, judged in terms of fatness or leanness when contracted, seems largely visual. The muscle tires soon and gives no cues so I have to rest more frequently than under other instructions."

7. "Judgment on the basis of the kinaesthetic sensations in the wrist. But this morning these sensations seem to be rather vague and indistinct."

9. "This morning the wrist kinaesthesis was clear and the judgment on this basis seemed to be fairly accurate."

13. "Judgment in terms of kinaesthesis in forearm. The visual imagery shifts to the forearm but is not so constant as in some other cases. The arm seems to tire more quickly in this instruction,—perhaps because I am better aware of the tired muscle when paying attention to it. The failure was due to a lapse of attention."

The comparisons according to instruction C were made on the basis of weight itself. In all but one report (1) stimulus attitude is definitely claimed. In the first and seventh divisions it is stated that judgments were made on all cues. There was some contamination from kinaesthesis in the eighth divi-

sion. Visual images of hand and weight were experienced under this instruction. This instruction was easier of fulfillment than others.

Attitude C

1. "Judgment much easier with this instruction. Am able to form judgment on the basis of all clues which are present, instead of limiting attention and judgment to one particular one. All of this makes me feel how much the stimulus attitude must have entered under former instructions." (A, first group.)

9. "Judgment in terms of weight. Physical and mental condition not so good this morning, so assurance as to judgments is rather lower than usual. The failure reported was due to lapse of attention."

17. "Stimulus attitude—but feel fairly certain of the judgments although I find my mind wandering at times. One seems to be able to judge weight so much more easily. Visual imagery scantily present."

M maintained B-attitude in greatest purity, with C and A following in order. (These ranks are found in Table X.) Of the 54 divisions contamination is reported for only seven. This indicates 87% purity of attitude.

O holds the first place in rank order as to fulfillment of instructions. Pressure on the finger-tips was the criterion of judgment under instruction A. Every report asserts obedience to instructions. In the eighth division stimulus attitude intruded. In the tenth division instructions were fulfilled "to the best of my ability." No imagery is reported.

Observer O: Attitude A

1. "I noticed the difference in instructions which called for pressure at the finger-tips—whereas in the former weight experiment the judgment of weight lifted was requested—and I concentrated my attention on the tips of the fingers. For each standard weight I found a judgment of 'pressure' and when the comparison weight came, the difference in pressure seemed very apparent. The failures reported were due to slow reaction time—delay in giving the judgment of the preceding comparison weight which made the lifting of the next standard weight practically automatic."

14. "The pressure of the finger-tips on the weight was the basis on which my judgments were made. Nothing unusual seems to occur during the experimentation. I keep my attention on the pressure on the finger-tips and report verbally."

18. "This series moved along much faster than the series this morning. It seems that the practice and rest aided in determining the judgments based on the intensity of the pressure sensations on the tips of the fingers."

O judged on the basis of kinaesthesis in the wrist under instruction B. This criterion is claimed in every written statement. In division 1 there was some admixture of the pressure criterion and in division 7 stimulus attitude "attempted to persist." Visual imagery of wrist was experienced in division eleven. Judgment at first seemed more difficult than in A.

Attitude B

1. "At first it was very hard to get away from the pressure idea which had been gone through immediately before this. As I proceeded I

began to note what seemed to me to be a definite greater or less degree of kinaesthesis in the wrist, and there was generally some small deciding factor which turned what might have been doubtful into definite results. This factor was the muscular feel which I can best describe as being from the wrist up to the fingers."

10. "By forcing or concentrating upon the kinaesthetic sensations in the wrist I feel that I made comparatively accurate results during the division. The change to this attitude from the stimulus attitude which preceded was not so hard to accomplish as I anticipated."

17. "The attitude this time was for the purpose of judging the kinaesthetic sensations in the wrist—a standard and a comparison weight. Fatigue seems to enter—I am thinking entirely of wrist movement, and the feeling toward the end of the series is that of semi-numbness."

In following instruction C this *S* made comparisons of the weights themselves. This standard of judgment was asserted in every one of the 18 protocols. There is no hint of any kind of contamination. No imagery is reported. This instruction was easier to follow than the other two.

Attitude C

1. "The experiment today was relatively easy for me, for in thinking of the weights I had little difficulty in distinguishing what to me were greater or less than the comparison weight. I made judgments purely on the basis of the objective stimulus."

10. "The stimulus attitude is by far the easiest method of judging the weights and the series of eight (rounds of the table) was completed with scarcely a sign of fatigue. One failure, reported near the end of the division, was due to a lapse of attention."

17. "Concentration was very poor today, having spent three days reading examination papers. The judgments were made on the basis of the weights themselves. I cannot give a high degree of assurance for these results."

O obeyed instructions best under C. A and B follow in order. Interference by foreign criteria is reported for but 3 divisions out of the 54. Therefore, he maintained purity of attitude to the extent of 94%.

S ranks fourth as to purity of attitude. Judgments under A were accomplished by means of concentrating attention on the tips of the fingers, sometimes the tip of the thumb alone. This attitude is claimed for every division except the first. In divisions 7, 8, 13, 14, 15, 16 and 18 the size of the area of the finger or thumb stimulated was the criterion. Stimulus attitude intruded in 4, 5, 14 and 18. There was an admixture of kinaesthesis in 2, 3, 6, 12 and 17. Rate of lift played a part in 1. Maximal attention was required throughout in order to maintain the necessary attitude. Assurance was low in much of the work. There was often difficulty in isolating the pressure patterns. In 9, 10, and 14 some judgments would have been different under other instructions. This *S* had some difficulty in keeping the rhythm with the metronome. Some judgments of equality expressed only inability to discriminate greater or less rather than positive decisions of equal.

Observer S : Attitude A

1. "There is a decided degree of difference in assurance of judgments of less, and too in the case of greater judgments. At times the hand seems almost to spring up and my judgment is unbesitatingly less; at others the pull at the finger-tips is distinctly greater. I find that the memory image of the standard weight fades rapidly, and especially in the first few rounds. There I could detect no subjective difference in the pull."

12. "As far as I know I adhered to the conditions of confining judgments to the pressure sensations at the finger-tips. But there may have been a few in which there was present a combination of these sensations (at the base of the thumb and index finger) from squeezing the weight in grasping. These might be confused with an extension of the pressure sensations along the fingers. Confidence was no more increased than in other sittings. There were no failures."

18. "This session, more than any others, I feel that my judgments were colored by stimulus attitude. I had little confidence in my judgments, but made them as well as I could on the basis of the mental processes (pressure sensations of the tips of the fingers). Again I find that I confine them to those (sensations) from the thumb in almost every case, and that the intensity is estimated in terms of extent of skin stimulated."

When B-instruction was given S used as the criterion of comparison the intensity of kinaesthetic sensations in the wrist as far as possible. All introspections report this basis of judgment. In divisions 1 and 5 fatigue interfered. Fatigue was rapid but offered no interference elsewhere. Visual imagery of the moving wrist played a part in 5. In 6, 15, 17 and 18 stimulus attitude forced itself in somewhat. Decisions would have been different if another attitude had been used in 6, 7, 10, 11, 12 and 14. "Other fields" claimed attention in 16. It was difficult to rule out foreign attitudes. Maximal attention was required at all times. Isolation of kinaesthetic criteria was enhanced at times by the *O* imagining that he was looking at the wrist (eyes were closed). There was some difficulty in keeping the rhythm. A good number of equality judgments were negative.

Attitude B

1. "When my attention is to be directed to the kinaesthetic sensations at the wrist I find that judgment of greater or less becomes extremely difficult, first, because of the difficulty of attending solely to the wrist sensations (by imagining I was looking at the wrist I made it a little less difficult), and secondly because of the slight discriminable difference in the sensations. In the early part of the division I was much less sure of my judgments, and equal was given often as representing no subjective difference. Fatigue was more noticeable when attending to these sensations and must have influenced my judgments."

7. "By imagining that I was fixating on the wrist itself I found no trouble in keeping the attention on the kinaesthetic sensations of the wrist. Fatigue seemed to set in more rapidly under these conditions, but not enough to interfere with judgments. Difficulty of discrimination was marked by the large number of equal judgments. And a number of the other judgments would have been different if I had been able to use any other criteria than the kinaesthetic sensations of the wrist. There were no failures."

16. "Immediately I experienced difficulty in isolating from the host of other sensations present only those of kinaesthesis at the wrist. Accordingly there are a large number of subjectively equal judgments. It requires a maximum of attention at all times to make any sort of judgment, and at times I find myself in other fields so that I cannot always discriminate with any definiteness."

Under stimulus instruction (C) S made his comparisons on the basis of the stimulus itself. Fulfillment of instructions is definitely claimed in every report. In 2, 3 and 9 kinaesthesis intruded. Rate of lift played a part in 5 and 8. No imagery is reported. This attitude was easiest to maintain. Maximal attention was not, as a rule, required. Many equal judgments were negative.

Attitude C

1. "I felt less sure of my judgment today than in other sittings, although this time I directed my attention strictly to the stimulus itself, disregarding sensations at the finger-tips, wrists, etc. After lifting a distinctly heavier weight the standard seemed quite light, and the comparison following it I judged most of the time to be heavier. My assurance was of a low degree on all but the very lightest weight."

9. "This morning I was in fine fettle and was able to finish the sitting with only two rests as I fatigued more slowly. The stimulus attitude, I find, requires less concentrated attention than the other attitudes. The second set of results (after the first rest) contained, it seemed, more equals than others, and I should account for these by the increased intensity of sensation at the wrist from the effects of fatigue which made discrimination difficult. There was one failure, due to the fact that I lost track of whether a weight was the stimulus or comparison one, due to wandering of attention."

17. "I followed instructions and made judgments on the basis of the weights themselves. At times I found it difficult to discriminate between them as the image of the standard weight faded so rapidly (probably lapsing of attention); this time there were many equality judgments."

According to the characterizations of judgment 44 out of 54 divisions were carried out with uncontaminated instructions. S's purity of attitude may therefore be expressed as 82%. Carriage of instructions for this *O* was best in attitude C, second in B, and least complete in A.

St ranks third as to excellence in the carriage of instructions, having a percentage of 87, the same as that of M. Comparison decisions in attitude A were arrived at by means of finger-tip pressure criteria. Pressure standards are definitely reported for divisions 1, 2, 3, 4, 5, 6, 7, 8, 10, 13, 14, 15, 17, and 18. In 1, 2, 7, 8, 10, 15, 16, 17 and 18 we find statements which, according to one interpretation, indicate somewhat of a mixed attitude, partaking of pressure and a muscular phase. We were inclined to rank St low on obedience to instructions for this reason; but when the matter was called to his attention in conference he insisted that he did not mean to infer that muscular sensations played any part in the judgments, and that the statements referred to were intended to define more carefully the pressure

experience which was focal. In the light of the *O*'s claims we have not graded him down in the percentile rating; but in view of the fact that M was a much more highly practised *O* than St, and had had a longer introspective training, we place St in third place as to excellence in the fulfillment of instructions.

Observer St: Attitude A

1. "The attempt made here was to attend to the sensation on the finger-tips. I tried to do this by judging the relative firmness with which I had to grasp the weights with my finger-tips to make them stay. It seemed that there was a difference in whether the weight was immediately in front of my hand or the least bit to the side."

4. "In judging from pressure on the finger-tips this pressure must be clearly differentiated from the pressure of the finger-tips on it; thus if the object is grasped more firmly in the one case than in the other the pressure sensation is greater. Pressure can best be judged if it is exerted exactly over the same surface from time to time, hence there is a noticeable difference when the weight is grasped with the hand in a different position.

16. "Judgments made from sensations on the finger-tips. Same difficulty was experienced here as formerly, *i. e.*, the manner of grasping has a great influence on the judgment; attempt was made to grasp them in as uniform a manner as possible. In some cases the assurance of judgment was very low."

With instruction B the *O* judged on the basis of kinaesthetic sensations in the wrist. This criterion is definitely claimed for all divisions except the second. Pressure sensations forced their way into 1, 3, 4, and 11. Totality of sensations intruded in 7, 8 and 15. There was no visual imagery. In some parts the weights were lifted high in order to clarify the kinaesthetic sensations.

Attitude B

3. "Particular attention was paid to any equality judgments. Seemed to have great difficulty today in judging wrist sensations, and excluding finger sensations."

9. "Kinaesthesis of wrist was used as a criterion. Difficult, but found by raising weights higher in the air each time the sensations became more clear and hence easier to judge. There did not appear to be nearly so much hang-over from stimulus to stimulus today."

17. "Sensations in the wrist were the criteria from which judgments were made. I worked with the wrist free, not allowing it to rest upon anything and I lifted the weights high in order to emphasize the wrist sensations. Toward the latter part I noticed a decreasing sensitivity to these sensations."

Judgments were based on the stimulus itself or the totality of sensation under instruction C. Stimulus attitude is specifically affirmed in every division except 8. No visual imagery is reported. No corruption of instructions is reported, unless summation of all sensations is to be so interpreted. But St's reports hardly justify us in so regarding it. High lifts helped to clarify judgments.

Attitude C

1. "A stimulus attitude was taken here. I take this to mean the summation of everything—is it heavier? There was a decided hang-over

here due to fatigue—the judgments on the whole, however, are easier to make than the others (A and B.)"

6. "There seemed to be somewhat of a carry-over from standard to comparison weight that was confusing here. This was not noticed until we had gone around the table two or three times. Generally speaking, judging from the stimulus attitude is much easier than from either of the others."

16. "Judgments made from the stimulus attitude. The blocks were lifted rather high and the judgments made from the totality of the sensations. Care was taken to have the hands as nearly as possible in the same position during the lifting of the standard and its comparison weight. Question here in my mind was always 'Is it heavier or lighter?'—not 'What are the different sensations?' "

A and C are without specific report of any admixture of foreign attitude. But since specific fulfillment of instructions is more frequently claimed under C we are rating this attitude first as to purity, with A and B following in order.

For the four *O*s combined there is an 87% purity of attitude. For several divisions in which the particular attitude is not specifically claimed it is, we believe, clearly implied, while particulars are dwelt upon. All regarded stimulus attitude as of greatest ease and the kinaesthetic most difficult. The disclaimers of contamination are really positive evidence for the fulfillment of instructions, since they express the ability of the *S*s concerned to distinguish between fulfillment and non-fulfillment of directions. While these percentile ratings by no means tell the entire truth, they are sufficiently indicative of the facts to reveal in a general way what happened. And although the contaminated divisions are but 13% of the entire amount of work, they really represent inconstancy of attitude to at least that extent.

C. The Relation of Qualitative and Quantitative Results

Table X is a record of the correlation of purity of attitude with statistical results. The results under the several instructions are ranked in order of preference with the highest excellence set opposite 1. The categories of highest preference are the greatest magnitude of *h* and P. S. E., the least value of the I. U., the least contamination of comparison attitude and the most pronounced effect of progressive practice. Practice effect is measured principally by varying degrees of progressive increase in accuracy (*h*) but also to some extent by the decreasing interval of uncertainty.

For M there is a full correspondence between purity of attitude and accuracy. As to I. U.'s (measures of sensitivity) the medium positions (2) are in harmony while the other two are reversed. The highest P. S. E. corresponds to attitude while the remaining values are reversed; B's are better than C's and A's. As regards practice effect C's under *h* are better than A's, but the B's are reversed. The I. U.'s show no effect.

The *h* of O shows a complete correlation with fulfillment of instructions. The I. U's correspond in B-attitude. A and C reverse positions, but are better than the B's. For the P. S. E.'s the A's are better than the B's but the C's are in opposition to one another. Under practice effect the I. U.'s are of no significance while the *h*-values correspond in that the A's are better then the B's but the C's occupy the two opposite extremes.

The correspondence between attitude and precision for S is not complete. The C's occupy first position while the A's and B's oppose one another in the two lower places. C's are better than A's and B's. The case with the I. U.'s is identical. For the P. S. E.'s the A's are in harmony while the B's and C's are in opposition. B's and C's are better than the A's. There is a complete correspondence as to the practice effect in the *h*-values, while under the I. U.'s the A's are opposite extremes and C's are better than B's.

St's figures present a full correlation between attitude and P. S. E.'s. As to precision the C's occupy first place while the other two are in opposition. With the I. U.'s there is no correspondence except that the A's fall into mid-position. The C-attitude yielded the most pronounced practice effect (*h*), and is in harmony with attitude, but the other two attitudes are at variance. No practice effect is shown by the I. U.'s except in C.

Practice effect will be noticed in the next Section. There is a complete correlation between precision and fidelity to instructions in the case of two *O*s and a pronounced tendency in the same direction on the part of the other two. We discover a strong tendency toward correspondence between attitude purity and I. U.'s on the part of one *O*, a slight tendency in the case of another, while the other two are practically without tendency. There are weak tendencies toward agreement of attitude purity and decreased effect of time error in the case of three *O*s and a complete correlation for the fourth. There is found a full correspondence between purity of attitude and degree of practice effect, as shown by the *h*-values, for one *O*. There are marked tendencies in the same direction on the part of the other *O*s. The strong general tendency toward correspondence between precision and purity of attitude is in harmony with the findings of Reid.

D. The Effect of Progressive Practice

An examination of the effect of progressive practice reveals a complete correlation with purity of attitude in the case of S, and a pronounced tendency in the case of St, while weaker tendencies in that direction are discernible in the statistical results of M and O. The relative amounts of increase in the value

of h in each of the three series are the sole determinants of practice effect considered here. An increase of such effect is found in every attitude and in the case of every *O*. It is least noticeable in the h_2 for St under instruction A. This fact stands in strong contrast to the results of Reid, in which intra-serial practice effect was discernible, with one exception, only in the cases of uncontaminated instructions. In a sense this result is in harmony with our correlation of purest attitude with greatest practice effect. But a wide discrepancy remains to be explained. In Reid's experiment all work under one instruction was completed before a new instruction was given, while in our investigation any group under any given instruction was followed by a group under another instruction. Between the taking of any two groups of judgments under any one instruction, there intervened 700 judgments under the two other attitudes. The conditions of our experiment do not, then, permit of the isolation of inter-serial practice effect. If there is inter-serial effect, it is mingled with the intra-serial influence. Without a doubt there are sufficient common factors in the three attitudes to secure that, with two heterogeneous groups interspersed between two homogeneous groups, the summation of inter-serial practice effect would show itself, added to the intra-serial effect, in the third group. It is natural, then, that we should find a larger amount of the progressive effect of practice in our own results. But in consideration of the relatively small amount of such influence found by Reid it is extremely doubtful whether these facts completely account for the discrepancy. In Reid's experiment the greatest degree of practice effect was discernible under the initial instruction. Within limits the same condition prevails in our own work, and is naturally to be expected. Our I. U's furnished no clue to the solution of this problem, since they either showed no effect whatever or were in inverted order. It should also be noted that Reid worked with both time orders while we worked with only the first time order,—a situation which tends to definitize the standard stimulus,—and these facts may also have some influence on the discrepancies of the results.

E. Failure Judgments

Account of failure judgments was taken with M and S after the first 2100 judgments had been given, and with O and St after 1050.

M reported 22 failures. Lapse of attention was given as the cause, except in one case, when it was mechanical. O gave 9 failure judgments. He accounted for 7 by lapse of attention, 1 by distraction upon opening his eyes when the memory of the standard weight failed, and 1 by confusion at the beginning of a division on account of the bell ringing on the odd count. S

had 15 failures. He explained 11 by mechanical interference or failure to keep the rhythm, 2 by lapse of attention, and 2 by distraction from noise in the adjoining room. St gave no failure judgment throughout the entire experiment.

Failure judgments are recorded for every comparison pair, but the weights of 88, 92 and 96 gm predominate. 25 were given under A-attitude, 6 under B, and 15 under C. These failures of judgment can not, so far as we can determine, be significantly correlated with any other facts. According to our instructions these failures were eliminated from the reports and calculations. They are so few in number that such an exclusion would hardly have any significant effect on the statistical results.

F. Significance of Difference

Table XI gives the indices of significance of difference between the several attitudes. These values are calculated for both *h*'s, for the I. U. and for the P. S. E. St, the *O* who ranks first in precision, has the lowest average significance of difference, while S, whose accuracy is fourth in order, has the largest average index. For O, 7 cases out of 12 show indices of significance of over .5000; M shows 4; S has 10 such; St has only 1. These results are exceedingly equivocal when one considers the values of each *S*, or when one compares the values from *O* to *O*. In a very few cases are the values great enough to be of any significance.

G. Tendency of Judgment

Every *O* gave more greater than less judgments. The difference amounts to 461 for O while for M it is but 88. St gave the smallest number of equality judgments. The highest number of such decisions was given by S. These figures furnish a direct check upon the measure of sensitivity. The *O* with the highest precision has the smallest number of 'equal' decisions to his credit, and conversely. 4515 correct judgments were given by St; 3704 by *O*; 3586 by M; and 3312 by S. The complete figures for tendency of judgment are found in Table IX. In the first column of this table are given the letters indicating the instructions under which the *O*s worked. In the second column, headed G-L, are given the totals of correct judgments when a lighter weight followed a greater. In the third column are similar totals of correct judgments when a greater weight followed a lighter. The totals of correct judgments, irrespective of the order of weights, are given in the fourth column. In the next three columns are given the number of judgments of greater, less and equal respectively.

H. The Comparison Consciousness

We present here a detailed account of the comparison consciousness as revealed by a series of introspections or reports.

Each of the four *O*s judged each pair of weights under each of the three instructions and immediately wrote a descriptive account of the criteria of judgment. This study was made after we had obtained the statistical results, with the reports already discussed, and was performed in an effort to check further and more minutely the constancy and purity with which the *O*s maintained the three attitudes. Judgment was made on the pair immediately preceding the pair of weights for which report was desired, but the processes involved in the second judgment only were reported. In this way the conditions for the study were rendered identical with those in the earlier part of the investigation. Each *O* gave 14 reports under each instruction, or 42 for the three attitudes. Three sample characterizations, taken from various stages of the work, accompany the account.

M in attitude A reports judgment made on the amount of area stimulated on the finger-tips for 8 pairs; that is, when the area for the comparison weight was less than for the standard the judgment was less. In three of these it is definitely stated that the amount of the area was held in mind visually (in one case pressure translated into area and compared). In the second judgment finger-tip pressure was thought of after the first pair. Three judgments showed scant consciousness and were on the meaning level. Vague kinaesthesis was hard to rule out in one case. Eight times marked visual imagery was reported. Kinaesthesis was a partial criterion in one decision. For two pairs pressure was so clear that it seemed *O* was living in the finger-tips.

Observer M: Attitude A

84-Less. "The lifting of the first pair of weights prepares me for the second. In lifting the first of the second pair of weights I try to get and hold in mind visually the amount of area stimulated on the end of the fingers, for this is the basis of judgment. In this case I find the area stimulated by the second weight of the pair to be less and so judged less."

88-Less. "Pressure patterns in second of the pair had less area, so less intense, so judgment less. Visual imagery marked. Whole thing mechanized. Scant consciousness. No consciousness of the stimulus attitude."

108-Greater. "The first weight of the introspective pair gave distinctly greater pressure pattern than the comparison weight preceding it, but the comparison weight of the introspective pair gave a still greater area of pressure on the finger-tips, so judged greater."

In the B-attitude (kinaesthesis in wrist) the *O* declared in the first report that he no longer used the old cues (pressure) and a new set had to be built up. In 7 cases he visualized the contraction of the forearm muscle; if it became fatter he judged greater, if less the judgment was less. In 9 comparisons there was visual imagery of the hand, table and weight. Imagery of the muscle appeared in 7 judgments. Seven times more kinaesthesis is mentioned. Once the judgment was on the basis of

the muscle pressing on the chair. The last judgment was reported as mechanized and immediate.

Attitude B

104-Less. "Judgment in terms of the muscle size taken in visual terms. The visual imagery of the table, arm and especially of one of the muscles in the forearm is marked. I feel it pull together and see its size and shape in imagery. The discrimination by this method seems much more crude than by other methods."

84-Less. "With the first weight of the introspective pair there is a definite pull in the forearm muscles,—with the second weight hardly any pull at all,—so I judge less. Visual imagery present but not focal."

100-Greater. "The judgment is getting mechanized so that more muscle sensation on second stimulus is immediately greater in report. Visual imagery, especially of the muscle—its size and shape in contraction."

In the C-attitude the judgments were definitely on weight for 5 pairs. Visual imagery of the hand, table and weights appeared in 10 comparisons. Kinaesthesis and pressure entered into 8 reports, kinaesthesis alone in two (played main part in three), and pressure alone in three. Eight reports show ease or difficulty of lift as a criterion, mingled with other criteria. Attitude was passive in two cases. In one judgment any helpful cues were used. Once a complex basis is reported. In one the judgment was checked with kinaesthesis. Once the *O* saw the muscle contract visually.

Attitude C

96-Greater. "Under this instruction I judge weight, using any cues that seem helpful. This judgment was made on a combination of finger-tip pressure and kinaesthesis in the forearm. There was more of these in the second weight, so I judge greater."

104-Less. "Judgment of less arrived at through finger-tip pressure and weight (weight meaning 'does it come up from the table or not'). This time the difference between the weights was small but enough for the judgment. Visual imagery not marked."

88-Equal. "I make these judgments in a passive way—just sit back and lift the weights and then decide which was heavier. In this case, however, I am sure that the judgment was largely in terms of finger-tip pressure; the two pressures being equal, I judged equal."

For O, the A-attitude yielded 13 cases in which the pressure criterion is reported, but there was an admixture of kinaesthesis in 8 comparisons. An effort to make an equal lift for the two weights is once reported.

Observer O: Attitude A

84-Less. "Less undoubtedly. The standard pulled on all of my fingers, and when I lifted the comparison weight the pressure I put on it made (seemed to make) the comparison weight much lighter."

96-Greater. "The pressure was greater—more of a pull at the tips of the fingers. By pull I mean that weight drew down the fingers and I naturally grasped hardly."

108-Greater. "Greater, the weight of the comparison stimulus made it necessary for me to grasp it tighter, and as a result the pressure was much greater. No doubt in my mind about this being greater."

TABLE I—Record of Judgments
Observer M

Instruction	84			88			92			96			100			104			108		
	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G
A	47	2	1	32	11	7	27	8	15	23	4	23	8	12	30	11	14	25	1	3	46
B	46	3	1	36	7	7	30	9	11	22	5	23	14	7	29	8	5	37	5	1	44
C	43	5	2	30	8	12	28	10	12	15	9	26	8	9	33	8	3	39	0	5	45
A	41	5	4	36	6	8	24	8	18	20	12	18	8	7	35	4	9	37	1	3	46
B	44	5	1	31	10	9	31	8	11	13	13	24	13	9	28	6	3	41	2	3	45
C	46	4	0	35	7	8	24	15	11	10	24	16	7	12	31	5	8	37	0	5	45
A	40	8	2	34	6	10	23	9	18	15	21	14	7	10	33	5	8	37	1	3	46
B	43	3	4	39	6	5	25	11	14	17	20	13	9	11	30	6	6	38	1	2	47
C	42	6	2	34	10	6	25	12	13	18	14	18	7	14	29	5	8	37	1	6	43
A	45	4	1	35	12	3	21	16	13	18	13	19	13	12	25	5	5	40	1	4	45
B	46	3	1	26	22	2	19	24	7	16	20	14	4	16	30	2	6	42	1	3	46
C	45	5	0	38	7	5	27	13	10	14	15	21	8	15	27	2	6	42	1	3	46
A	44	6	0	31	16	3	22	18	10	14	24	12	8	14	28	4	15	31	1	4	45
B	48	2	0	34	12	4	20	26	4	15	22	13	7	16	27	1	7	42	1	2	47
C	43	4	3	35	6	9	20	21	9	14	16	20	10	11	29	2	9	39	1	2	47
A	43	7	0	30	18	2	25	13	12	13	10	27	7	22	21	2	10	38	2	5	43
B	46	2	2	37	10	3	21	15	14	13	21	16	7	23	20	3	13	34	2	5	4
C	44	6	0	31	16	3	18	20	12	13	21	16	7	12	31	4	15	31	1	1	48
TOTALS A	260	32	8	198	69	33	142	72	86	103	84	113	51	77	172	31	61	208	7	22	271
TOTALS B	273	18	9	203	67	30	146	93	61	96	101	103	54	82	164	31	46	223	12	20	268
TOTALS C	263	30	7	203	54	43	142	91	67	84	99	117	47	73	180	26	49	225	4	22	274

TABLE II
Observer O

Instruction	84			88			92			96			100			104			108		
	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G
A	46	4	0	33	8	9	24	18	8	23	15	12	7	13	30	1	6	43	0	1	49
B	44	4	2	30	12	8	29	9	12	16	15	19	14	11	25	6	6	38	2	5	44
C	48	1	1	33	13	4	28	12	10	23	15	12	6	16	28	3	9	38	1	2	47
A	47	3	0	38	6	6	30	9	11	16	18	16	7	18	25	7	8	35	0	1	49
B	43	5	2	39	7	4	31	9	10	17	13	20	10	9	31	3	11	36	0	1	49
C	48	0	2	42	3	5	29	7	14	21	7	22	11	15	24	6	8	36	3	2	45
A	42	6	2	39	6	5	36	10	4	11	17	22	5	15	30	5	10	35	0	2	84
B	46	3	1	32	10	8	21	20	9	16	9	25	5	16	29	3	7	40	0	6	44
C	44	6	0	34	8	8	31	11	8	13	12	25	7	14	29	4	9	37	0	2	48
A	48	1	1	28	12	10	26	15	9	11	15	24	8	12	30	4	7	39	0	1	49
B	47	1	1	35	9	6	20	15	15	14	11	25	5	12	33	2	11	37	0	1	49
C	45	4	1	38	6	6	27	12	11	9	22	19	2	16	32	1	8	41	1	2	47
A	46	4	0	34	13	3	20	18	12	9	12	29	5	15	30	1	8	41	0	6	44
B	46	4	0	36	12	2	18	21	11	8	18	24	2	19	29	4	9	37	0	1	49
C	48	2	0	26	18	6	24	17	9	7	24	19	1	16	33	0	11	39	0	1	49
A	48	1	1	37	12	1	19	15	16	14	13	23	3	14	33	2	9	39	0	1	49
B	45	4	1	33	14	3	21	15	14	10	15	25	6	7	37	2	8	40	2	3	45
C	48	0	2	32	11	7	16	19	15	13	18	19	3	14	33	1	14	35	0	4	46
TOTALS A	277	19	4	209	57	34	155	85	60	84	90	126	35	87	178	20	48	232	0	12	288
TOTALS B	271	21	8	205	64	31	140	89	71	81	81	138	42	74	184	20	52	228	3	17	280
TOTALS C	281	13	6	205	59	36	155	78	67	86	98	116	30	91	179	15	59	226	5	13	282

TABLE III
Observer S

Instruction	84			88			92			96			100			104			108		
	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G
A	38	10	2	33	10	7	28	11	11	11	15	24	5	22	23	6	7	37	2	7	41
B	38	10	2	26	16	8	28	15	7	14	19	17	11	14	25	8	15	27	2	13	35
C	37	8	5	35	9	6	24	13	13	11	16	23	2	18	30	2	8	40	3	3	44
A	35	8	7	27	16	7	14	25	11	13	19	18	6	23	21	5	21	24	5	7	38
B	35	9	6	23	12	15	21	9	20	16	13	21	11	10	29	7	16	27	4	11	35
C	42	2	6	31	6	13	28	11	11	11	12	27	11	12	27	3	16	31	0	5	45
A	42	7	1	28	13	9	19	18	13	16	15	19	9	21	20	9	8	33	5	3	42
B	33	13	4	28	15	7	15	15	20	10	20	20	12	16	22	5	16	29	2	7	41
C	46	2	2	40	6	4	26	13	11	13	16	21	6	10	34	6	7	37	0	1	49
A	37	12	1	33	14	4	21	20	9	12	15	23	6	12	32	1	17	32	3	7	40
B	37	10	3	34	10	6	18	19	13	14	15	21	6	18	26	4	10	36	3	6	41
C	47	2	1	35	11	4	28	13	9	20	9	21	10	10	30	5	11	34	3	1	46
A	39	11	0	29	11	10	17	20	13	14	23	13	6	18	26	6	12	32	0	8	42
B	36	12	2	36	6	8	29	16	5	21	12	17	10	13	27	13	12	25	5	6	39
C	45	5	0	39	8	3	26	15	9	12	14	24	11	11	28	3	6	41	2	2	47
A	39	8	3	30	14	6	22	17	11	17	11	22	4	13	33	6	9	35	4	10	36
B	41	7	2	32	14	4	24	13	13	16	21	13	12	11	27	5	14	31	0	4	46
C	48	1	1	36	8	6	26	10	14	19	15	16	9	10	31	1	8	41	0	1	49
TOTALS A	460	56	14	180	77	43	121	111	68	83	88	119	36	109	155	33	74	193	19	42	239
TOTALS B	220	61	19	179	73	48	135	87	78	91	100	109	62	82	156	42	83	175	16	47	237
TOTALS C	265	20	15	216	48	36	158	75	67	86	82	132	49	71	180	20	56	224	7	13	280

TABLE IV
Observer St

Instruction	84			88			92			96			100			104			108		
	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G	L	E	G
A	47	0	3	43	0	7	25	1	24	21	0	29	8	0	42	4	1	45	2	0	48
B	2	6	41	41	1	8	25	2	23	17	0	33	10	3	37	3	1	46	1	1	48
C	45	0	5	38	4	8	25	2	23	12	5	33	5	4	41	1	3	46	1	1	48
A	48	0	2	46	0	4	35	0	15	19	1	30	9	0	41	5	0	45	1	0	49
B	49	0	1	47	0	3	32	0	18	23	2	25	8	12	30	3	2	45	1	1	48
C	48	0	2	45	3	2	40	1	9	23	4	23	13	17	20	8	3	39	0	2	48
A	48	2	0	43	1	6	41	0	9	25	2	23	11	12	27	5	0	45	1	0	49
B	48	0	2	43	0	7	42	0	8	24	2	24	8	2	40	3	2	46	0	0	50
C	50	0	0	43	0	7	40	1	9	24	4	22	9	15	26	5	1	44	1	0	49
A	47	1	2	48	0	2	36	1	13	23	1	26	12	8	30	7	0	43	4	0	46
B	49	0	1	42	2	6	32	0	18	30	0	20	7	11	32	4	1	45	5	0	45
C	50	0	0	47	0	3	48	0	2	31	2	17	7	18	25	3	1	46	1	1	48
A	50	0	0	45	0	5	41	0	9	25	0	25	4	14	32	6	0	44	1	0	49
B	50	0	0	46	0	4	36	1	13	24	0	26	8	8	34	1	0	49	1	0	49
C	48	1	1	43	0	7	33	0	17	31	0	19	3	9	38	2	0	48	2	0	48
A	48	0	2	43	0	7	36	0	14	23	0	27	7	16	27	1	1	48	2	1	47
B	50	0	0	41	0	9	43	0	7	29	0	21	3	15	32	2	0	48	1	0	49
C	49	0	1	41	0	9	42	0	8	23	0	27	2	12	35	2	0	48	2	0	48
TOTALS A	288	3	9	268	1	31	214	2	84	136	4	160	51	50	199	28	2	270	11	1	288
TOTALS B	288	2	10	260	3	37	210	3	87	147	4	149	44	51	205	16	5	279	9	2	289
TOTALS C	290	1	9	257	7	36	228	4	68	144	15	141	40	75	185	21	8	271	7	4	289

TABLE V—Principal Measures

Observer M

	Instruction	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
	A	0.075	0.075	93.71	98.85	5.14	96 .26
	B	0.074	0.117	94.93	97.58	2.65	96.55
	C	0.073	0.079	92.22	96.49	4.27	94.44
	A	0.085	0.077	92.34	96.80	4.46	94.46
	B	0.078	0.085	92.82	97.35	4.53	95.18
	C	0.099	0.053	91.99	98.24	6.25	94.17
	A	0.103	0.078	91.93	97.28	5.35	94.23
	B	0.087	0.085	91.80	97.99	6.19	95.44
	C	0.085	0.081	92.19	98.42	6.23	95.23
	A	0.076	0.104	92.58	98.37	5.79	95.93
	B	0.054	0.093	89.26	93.40	11.17	96.35
	C	0.088	0.118	91.32	97.87	6.55	95.56
	A	0.086	0.091	91.49	99.89	8.40	95.75
	B	0.103	0.115	91.87	98.98	7.11	95.62
	C	0.090	0.091	91.64	98.59	6.95	95.14
	A	0.088	0.082	91.35	98.63	7.28	94.86
	B	0.095	0.079	92.15	99.86	7.71	95.65
	C	0.087	0.090	91.01	98.92	7.91	95.03
TOTALS	A	0.514	0.507	553.40	589.82	36.42	571.49
	B	0.491	0.574	552.83	585.16	39.36	574.79
	C	0.522	0.512	551.37	588.53	38.16	569.57
AVERAGES	A	0.085	0.084	92.23	98.30	6.07	95.23
	B	0.082	0.095	92.13	97.51	6.56	95.80
	C	0.087	0.085	91.89	98.09	6.36	94.93

TABLE VI
Observer O

Instruction	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
A	0.097	0.099	92.61	97.67	5.06	95.15
B	0.092	0.079	92.80	98.39	5.59	95.37
C	0.098	0.100	93.05	98.88	5.83	95.99
A	0.095	0.086	93.67	98.93	5.26	96.17
B	0.094	0.095	93.33	98.86	5.53	96.11
C	0.094	0.081	94.79	98.34	3.55	96.44
A	0.102	0.096	93.15	98.37	5.22	95.69
B	0.098	0.086	91.64	97.64	6.00	95.53
C	0.094	0.093	92.46	98.77	6.31	95.60
A	0.089	0.090	91.57	97.01	5.44	94.35
B	0.110	0.091	91.74	96.80	5.06	94.03
C	0.135	0.099	92.04	96.43	4.39	94.32
A	0.117	0.091	91.11	97.51	6.40	93.91
B	0.108	0.101	91.28	97.90	6.62	94.47
C	0.135	0.086	90.33	97.08	6.76	92.96
A	0.108	0.102	92.16	97.04	4.88	94.53
B	0.094	0.097	91.27	96.95	5.68	94.15
C	0.119	0.082	90.89	96.41	5.52	93.14
TOTALS A	0.608	0.564	554.27	586.53	32.26	569.79
TOTALS B	0.596	0.549	552.06	586.54	34.48	569.66
TOTALS C	0.675	0.541	553.56	585.91	32.36	568.45
AVERAGES A	0.101	0.094	92.21	97.75	5.37	94.96
AVERAGES B	0.099	0.091	92.01	97.75	5.74	94.94
AVERAGES C	0.112	0.090	92.22	97.65	5.39	94.74

TABLE VII
Observer S

Instruction	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
A	0.077	0.074	91.21	98.82	7.61	94.94
B	0.062	0.061	91.02	101.64	10.62	96.28
C	0.082	0.078	90.64	90.64	5.36	93.24
A	0.057	0.052	88.24	101.66	13.42	94.85
B	0.052	0.040	87.45	99.64	11.56	93.45
C	0.083	0.060	91.74	97.66	5.82	94.21
A	0.059	0.068	90.81	99.64	8.83	95.52
B	0.057	0.058	88.12	99.41	11.29	93.82
C	[illegible]	[illegible]	[illegible]	97.59	4.45	95.14

TABLE VII
Observer S

Instruction	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
A	0.077	0.074	91.21	98.82	7.61	94.94
B	0.062	0.061	91.02	101.64	10.62	96.28
C	0.082	0.078	90.64	90.64	5.36	93.24
A	0.057	0.052	88.24	101.66	13.42	94.85
B	0.052	0.040	87.45	99.64	11.56	93.45
C	0.083	0.060	91.74	97.66	5.82	94.21
A	0.059	0.068	90.81	99.64	8.83	95.52
B	0.057	0.058	88.12	99.41	11.29	93.82
C	0.110	0.097	93.05	97.50	4.45	95.14
A	0.082	0.078	90.50	98.83	8.33	94.65
B	0.073	0.076	90.39	98.69	8.30	94.63
C	0.081	0.113	95.08	98.45	3.37	97.04
A	0.071	0.066	89.97	92.01	2.04	90.95
B	0.056	0.066	93.27	101.13	7.86	97.61
C	0.098	0.090	92.86	96.58	3.72	94.63
A	0.068	0.068	90.91	98.94	7.53	94.92
B	0.073	0.082	91.99	99.50	7.51	95.31
C	0.111	0.132	93.06	97.12	4.06	95.27
TOTALS A	0.414	0.406	551.64	589.90	47.76	565.74
TOTALS B	0.373	0.383	552.24	599.38	57.14	571.10
TOTALS C	0.565	0.570	556.43	583.26	26.83	569.53
AVERAGES A	0.069	0.068	91.94	98.31	7.96	94.29
AVERAGES B	0.062	0.064	92.04	99.89	9.52	95.18
AVERAGES C	0.094	0.095	92.74	97.21	4.47	94.92

S	C	0.710	0.631	571.25	579.50	8.25	574.71
AVERAGES	A	0.110	0.104	95.17	96.51	1.33	95.81
	B	0.116	0.104	94.99	96.10	1.11	95.52
	C	0.118	0.105	95.21	96.58	1.37	95.78

TABLE IX
Tendency of Judgment

Instruction	Correct Judgments: Order G-L	Correct Judgments: Order L-G	Totals	Entire Number of Judgments: Greater	Less	Equal
			Observer M			
A	703	478	1181	792	891	417
B	718	496	1214	858	815	427
C	692	499	1191	913	769	418
Totals	2113	1473	3586	2563	2475	1262
			Observer O			
A	725	520	1245	922	780	398
B	696	508	1204	940	762	398
C	747	508	1255	912	777	411
Totals	2168	1536	3704	2774	2319	1207
			Observer S			
A	614	432	1046	831	702	567
B	625	412	1037	822	745	533
C	725	504	1229	934	801	365
Totals	1964	1348	3312	2587	2248	1465
			Observer St			
A	904	558	1462	1041	996	63
B	906	568	1474	1056	974	70
C	919	560	1479	999	987	114
Totals	2729	1786	4415	3096	2957	247

TABLE X

Relation between Qualitative and Quantitative Results

		Purity of Attitude	h	I. U	P. S. E	Practice Effect	
						h	I. U
M	1	B	B	A	B	C	—
	2	C	C	C	A	A	—
	3	A	A	B	C	B	—
O	1	C	C	A	A	A	—
	2	A	A	C	B	B	—
	3	B	B	B	C	C	—
S	1	C	C	C	B	C	A
	2	B	A	A	C	B	C
	3	A	B	B	A	A	B
St	1	C	C	B	C	C	C
	2	A	B	A	A	B	—
	3	B	A	C	B	A	—

TABLE XI

Indices of Significance of Difference

Observer M

I. U.,	A & B,	0.2641	P. S. E.,	A & B,	0.5254
I. U.,	A & C,	0.2893	P. S. E.,	A & C,	0.3242
I. U.,	B & C,	0.1233	P. S. E.,	B & C,	0.6786
h_1,	A & B,	0.7904	h_2,	A & B,	0.3340
h_1,	A & C,	0.0752	h_2,	A & C,	0.1656
h_1,	B & C,	0.6121	h_2,	B & C,	0.4739

Observer O

I. U.,	A & B,	0.8604	P. S. E.,	A & B,	0.0269
I. U.,	A & C,	0.0054	P. S. E.,	A & C,	0.2691
I. U.,	B & C,	0.5337	P. S. E.,	B & C,	0.2385
h_1,	A & B,	0.5337	h_2,	A & B,	0.2893
h_1,	A & C,	0.6515	h_2,	A & C,	0.7753
h_1,	B & C,	0.7371	h_2,	B & C,	0.8692

Observer S

I. U.,	A & B,	0.6515	P. S. E.,	A & B	0.5778
I. U.,	A & C,	0.9766	P. S. E.,	A & C,	0.6194
I. U.,	B & C,	1.0000	P. S. E.,	B & C,	0.1814
h_1,	A & B,	0.8373	h_2,	A & B,	0.1073
h_1,	A & C,	0.9996	h_2,	A & C,	0.6046
h_1,	B & C,	1.0000	h_2,	B & C,	0.6008

Observer St

I. U.,	A & B,	0.2843	P. S. E.,	A & B.,	0.3389
I. U.,	A & C,	0.0591	P. S. E.,	A & C,	0.0269
I. U.,	B & C,	0.4651	P. S. E.,	B & C,	0.2385
h_1,	A & B,	0.0054	h_2,	A & B,	0.8000
h_1,	A & C,	0.0215	h_2,	A & C,	0.4651
h_1,	B & C,	0.0484	h_2,	B & C,	0.1551

TABLE VIII
Observer St

	Instruction	h_1	h_2	S_1	S_2	Interval of uncertainty	Point of subjective equality
	A	0.101	0.099	93.96	94.12	0.16	94.04
	B	0.092	0.089	93.01	93.76	0.75	93.32
	C	0.109	0.103	92.06	93.63	1.57	92.68
	A	0.116	0.119	95.08	95.16	0.08	95.11
	B	0.126	0.150	95.04	95.50	0.46	95.29
	C	0.105	0.097	96.35	98.66	2.31	97.41
	A	0.113	0.108	95.22	97.18	1.96	96.13
	B	0.125	0.124	95.45	98.19	2.74	96.81
	C	0.109	0.101	95.77	97.34	1.57	96.53
	A	0.098	0.099	96.15	97.19	1.04	96.67
	B	0.094	0.092	95.63	96.71	1.08	96.16
	C	0.153	0.110	96.85	98.22	1.37	97.42
	A	0.125	0.105	95.79	98.92	3.13	97.22
	B	0.132	0.105	95.25	95.96	0.71	95.56
	C	0.113	0.110	95.16	95.74	0.58	95.45
	A	0.109	0.096	94.89	96.48	1.59	95.67
	B	0.126	0.109	95.59	96.51	0.92	96.00
	C	0.121	0.110	95.06	95.91	0.85	95.45
TOTALS	A	0.662	0.626	571.09	579.05	7.96	574.84
	B	0.695	0.624	569.97	576.63	6.66	573.14
	C	0.710	0.631	571.25	579.50	8.25	574.71
AVERAGES	A	0.110	0.104	95.17	96.51	1.33	95.81
	B	0.116	0.104	94.99	96.10	1.11	95.52
	C	0.118	0.105	95.21	96.58	1.37	95.78

Under instruction B this *S* reported kinaesthetic attitude in 13 comparisons, 9 of which refer to wrist muscles. Rate of movement intruded once. Kinaesthesis was especially clear in two cases. High assurance is reported in three decisions and low in one judgment.

Attitude B

84-Less. "The kinaesthetic sensation was less. The thing I think about is the wrist movement, and there was not so strong a sensation produced while lifting the comparison weight."

108-Greater. "Greater. The kinaesthesis is more clear to me as I progress. I have forgotten about correctness of judgments, *i. e.*, wondering whether I am right or wrong. The kinaesthetic sensation begins to stand out."

108-Greater. "Greater—4 to 1 on it. The pull here seems vivid. I visualize the wrist performing more work. Judgment based on that feeling of kinaesthesis in the wrist."

Under the C-attitude the *O* reports that he judged weight itself in all but one case. In 3 cases other sensations were said not to be in mind, and in two others it is definitely claimed that the judgments were not on kinaesthesis or pressure. There were two cases of exceptionally high assurance. Once he speaks of the stimulus attitude as the common attitude. Visual imagery is reported but once.

Attitude C

104-Greater. "Greater—more weight involved and a pull felt as I lifted the weight. The stimulus, *i. e.*, the weight is thought of—and my judgment is made on that basis."

100-Greater. "The judgment of the first comparison weight was greater, the second pair less. The weights themselves determine this, rather than any pull or pressure in hand or fingers. It seems that this is the easiest method of judging weights."

96-Equal. "I am not aware of the pressure or kinaesthetic sensations as I am under other conditions. I feel that the judgment is made on the weight itself."

S, while working under the pressure instructions (A), definitely claimed pressure sensations as the criterion in 9 cases. There was visual imagery of the hands lifting weights, etc., in 6 comparisons. Attention was given to the thumb chiefly in 8 judgments. In four cases it was difficult to rule out stimulus attitude. Kinaesthesis entered into two judgments. There was expectancy for less in three cases. Two times it is reported that the memory image faded rapidly. Once the *S* imaged the finger-tips. There was trouble with the rhythm once. Vocimotor images of the judgment word were experienced twice. Once there was unlocalized strain.

Observer S: Attitude A

92-Less. "I judged the first or practice weight as less, and wondered why my result had been different from before. There seemed then to be an expectancy of less for the next pair. I had a visual image, hazy and indistinct, it is true, of the lifting thumb, in particular, depressed because of

lifting the weight. Accompanying it was a feeling of cold. The intensity of the pressure sensations seemed less."

104-Equal. "The memory image of the standard weight—not particularly intense under this attitude—fades rapidly, and this time I could not distinguish the image of the pressure sensations of the comparison weight from it. I felt doubtful, but as I could not distinguish the intensities, one from the other, I reported equal."

96-Less. "I seem to have a tendency to judge the sensation resulting from the lifting of the second weight as less, although at the time of lifting the first it seemed to me that the intensity was not great. Yet clearly the sensations of pressure, this time, again, chiefly in the thumb, but partially in the other fingers, did seem of less intensity. Certainly, for me the memory image under this attitude fades rapidly."

Under instruction B this *O* reports kinaesthetic attitude in 13 comparisons, in 6 of which the sensations spread over the hand, wrist and back of the hand. The stimulus attitude entered into three decisions. Twice there was a negative equality decision. There was a vocimotor image of the judgment word in one case. Once expectancy of a greater or less is mentioned. In connection with four pairs there was trouble to keep the rhythm. In three comparisons there was visual imagery of the hand lifting. Pressure intruded one time.

Attitude B

84-Less. "I have always found it difficult in these judgments to entirely eliminate the stimulus attitude, but here as an aid I deliberately endeavored with some success to image visually my hand only, *i. e.*, without the weight, in the process of lifting. Then with attention fixed,—and it is as if I focussed my eyes on the wrist itself,—on the intensity of the kinaesthetic sensations, I judged. The pattern is spread over the upper surface of the wrist, and in particular intensity over the back of the hand. That is, the lesser extent of the sensations, localized in the wrist, arising from the second stimulus, gave rise to the verbal report of less."

92-Less. "The kinaesthetic sensations were localized this time quite clearly in the wrist, extending, however, in the second case but slightly up the hand. And as I base intensity here rather on extensity, I reported less."

100-Equal. "This time I lifted more slowly, apparently increasing the intensity and duration of the kinaesthetic sensations. All four of the weights lifted apparently aroused equal sensations, and I gave an equal report. I wondered at the time whether I was following the rhythm in lifting the comparison stimulus."

While judging in the stimulus attitude (C) S reports the use of definite weight attention in 5 cases. Kinaesthesis entered in 6 cases. The hand was visualized in 6 judgments. Once there was a positive equality decision. A vocimotor image of the judgment word was experienced three times. There were 7 cases of rhythm trouble. Rate of movement influenced one comparison, and the 'general feel' decided in one.

Attitude C

104-Greater. "The time or rhythm of lifting seemed to be increased. I was hurried, in fact, I neglected to give judgment of the first pair because I was concentrating on the second. I visualized my hand in the down motion, and in lifting the standard weight. Then I hurried to the compar-

ison, not too sure of the rhythm, and immediately had a vocimotor image of 'greater', which was the judgment given."

88-Less. "I was wondering whether the second pair would be as easy to discriminate as the practice pair, when suddenly I found that I had lifted the stimulus weight, and visualized myself replacing it. The comparison weight seemed to 'jump up' in comparison, and I called it less."

108-Greater. "In assuming this attitude I had a visual image of lifting the weights, and, in fact, find such a visualization a distinct aid in keeping the stimulus attitude. Relatively the standard weight felt heavy, *i. e.*, my hand came up with more effort, more feeling of strain than with a less judgment. Then I seemed to be set for a less judgment, but the comparison weight this time felt heavier and I gave the judgment greater."

St, working in the A-attitude, definitely claims pressure criteria in 12 protocols. In four decisions of this number other sensations are said to have been ruled out. Kinaesthesis entered in two cases. For 6 judgments the weights were barely lifted from the table.

Observer St: Attitude A

84-Less. "Pressure sensations on the tips of fingers attended to. Question here was how much pressure was experienced on the finger-tips as compared with the standard."

88-Less. "When holding the block with sufficient firmness to keep it from falling the pressure on finger-tips was greater. Care must be taken not to squeeze as I believe any results could be obtained if the degree of firmness is not more or less calibrated."

100-Greater. "Pressure sensations on the finger-tips were more intense than in the standard. Barely lifted clear of the table. Other sensations ruled out.'"

In the kinaesthetic attitude (B) the *O* claims kinaesthesis in the wrist as the basis of judgment in every judgment. In 8 of them a high lift brought out the kinaesthetic sensations and in one it is said to have eliminated the other sensations. Once there was difficulty in ruling out other sensations. In two cases a slower lift helped.

Attitude B

84-Less. "Sensations localized in the wrist less than in standard. Weights were lifted high with a large movement of the wrist—this seems to accentuate those sensations and repress others.

92-Less. "Weights not lifted so high. Judgment much harder as it was harder to rule out other sensations, however, these were ruled out as much as possible and judgment made."

104-Greater. "I lifted more rapidly and the judgment made was heavier,—made from intensity of sensations localized in the wrist."

S claims that under attitude C the criterion of judgment was the totality of sensations in 10 comparisons, but in 8 of these he states that it is not an additive affair, but a unit. Three reports say that the answer is to the question "Is it lighter or heavier?" Two claims are made for an immediate rather than an analytic judgment. In two cases local sensations interfered. General feeling is given as the basis in two comparisons, and the fact that the weight came up easier was the criterion in two others.

Attitude C

84-Less. "I lifted the standard weight and compared the totality of sensations from it with the comparison weight. Here everything was fused together in general judgment, not sensations of finger-tips + wrist + etc., but the totality."

108-Greater. "As soon as the weight is lifted the judgment comes into mind. No attempt at analysis. It seems to me that this is the way judgments are usually made."

100-Equal. "The judgment here was made by a process of inference. The previous one was much lighter and there was so little difference that I thought the table had not been moved; the totality of sensations seemed identical."

No one of our *O*s was able completely to follow instruction A. Even when one judgment only was made, kinaesthetic sensations intruded to a greater or less degree for every *O*. Visual images were prominent in the comparisons of M and S. The thumb was prominent for S, and he was troubled by failure to keep the rhythm as well as by the rapid fading of the memory image. All seem to have used pressure as the main criterion, but not as the sole basis. Areal pressure determined the issue for M and S.

For the kinaesthetic attitude also there is contamination reported by every *O*, though it is not the same in every case. All used the kinaesthetic basis primarily. Pressure interfered for M and St. Rate of movement influenced the decisions of O. Stimulus attitude intruded into the judgments of S. Visual imagery was prominent in the judgment processes of M and S, M visualizing the contraction of a forearm muscle. S again had trouble with the rhythm.

Even stimulus attitude, by some designated as the common attitude of judgment, was kept pure by but one *S*. All used plain weight as the principal basis of decision. M and S used kinaesthetic sensations to some extent. M used pressure also. Unspecified local sensations interfered in the case of St. He used totality of sensations (a unit) as the main criterion, identifying the totality with weight itself. Visual imagery entered into the decisions of M, S and O. O alone reports no contamination. The complex nature of the comparison consciousness in this attitude as revealed by the characterizations corresponds in a general way to the findings of Fernberger.[19]

The results of our specific study of the comparison consciousness correspond, with few exceptions, to the information gained in the earlier part of the present experiment. In attitude A O reported no contamination in the earlier reports, while M and S reported admixture of stimulus attitude. In B-attitude there is but one noteworthy exception, the influence of rate of

[19]S. W. Fernberger, An Introspective Analysis of the Process of Comparing, *Psych. Mon.* 26, 1919, 95-103.

movement for O. Under the stimulus attitude there are several exceptions. St reported no contamination in the earlier reports while M was influenced by fingertip pressure.

It is of interest, at this point, to make comparison of our accounts of the comparison consciousness, under the several instructions, with the tentative analysis offered by Reid, as well as to check the reports of his *Os* with that analysis. Under the stimulus attitude he believes that an *O* may judge either in terms of projected pressure or projected kinaesthesis. His *Os* reported, under this instruction, the following criteria of judgment: weight (something to lift), kinaesthesis, pressure and rate of movement. While there is no mention of the projection of sensations in the reports (even that of Reid as *O*) his analysis is probably justifiable to some extent, since an objective criterion is involved. The reports of our *Os* claim as criteria weight, kinaesthesis, pressure and rate of movement; but in most cases it is sheer weight, with a number of assertions that sensations were not involved. Certainly the suggested analysis cannot cover nearly all cases. We are compelled by our protocols to add at least one other category, namely that of weight without reference to sensation. This "sheer weight" without reference to sensation is undoubtedly the every-day, man-of-the-street meaning level. It is a process in which meaning maintains such a large place in consciousness that the specific and minute sensations underlying such meanings are insignificant as compared to the meanings themselves.

It is suggested by Reid that under the kinaesthetic instruction there are two possible criteria, a projected kinaesthesis or a process attitude so complex as to baffle the *O*. The reports of his *Os* show the use of wrist kinaesthesis and rate of movement. There is nowhere reference to the projection of sensations. Reid, as *O*, states: "It seemed impossible to judge in terms of wrist only." Our own *Os* reported as bases of judgment kinaesthesis, pressure, weight, and rate of movement, with kinaesthesis predominating. There was visual imagery in the earlier judgments. There is nothing in the reports to indicate projection, except in the cases in which weight was used. The complex process attitude was probably used in those cases in which pressure and kinaesthesis are indicated. But the majority of comparisons, in which kinaesthesis alone is reported, are not accounted for in the suggested analysis. We cannot agree that it is impossible to isolate kinaesthetic sensations, for every one of our *Os* did isolate them in a large proportion of judgments. We, therefore, add the criterion of fulfilled kinaesthetic instructions.

The tentative analysis, to which we have referred, suggests two possibilities under pressure instruction, namely decision on the basis of projected pressure or judgment with confused process attitude. Reid's *Os* reported under this instruction that they judged on the basis of pressure, weight, strain, and the quality, extent, and intensity of pressure. There is no suggestion of projecting any sensations, though in the case of the weight criteria it is not improbable. Our own *Os* state that they judged by the criteria of pressure (preeminently), weight, kinaesthesis, and visual images of the areas stimulated. Only the weight criteria may be said to lend themselves to the projection interpretation. Confused process attitude seems to be a plausible explanation of the combined use of pressure and kinaesthesis. Since there are many judgments which do not fall into any of these classifications, there must be at least one other criterion, *i. e.*, pressure patterns alone. The majority of our introspections testify to this type of basis.

Summary

We have discovered a strong tendency toward correspondence between carriage of instructions and accuracy of judgment. For two *O*s there is a complete correlation. The relationship between purity of attitude and practice effect in the several series is uncertain, although in the case of one *S* there is a full correlation. The conditions of our experiment, however, were not favorable to sharp differentiation in this respect. The failure to discern any noteworthy correspondence between purity of attitude and the I. U.'s and the P. S. E.'s is at first glance disappointing. Equally disappointing seems to be the case of the relative order of the size of the I. U.'s and the P. S. E.'s under the three instructions for the four *O*s (one with another). But it is difficult to see how one could have expected any such correlation with results of such totally different sense modalities as pressure and kinaesthesis, when compared with each other or when compared with the equivocal stimulus attitude. It would be equally reasonable to compare limens for visual and auditory sensations with each other and with a mixed sensory attitude.

However, the correlation which is found between purity of attitude and index of precision seems as reasonable as one might have expected under equivocal conditions, and indicates the necessity of purity of attitude in future psychophysical experiments. This may eventually mean the abandonment of a rapid series of judgments which has previously been the ideal in psychophysics. The advantage of such an instrument as the turning-top table for lifted weights is that the *E* acquired a sufficiently large number of judgments for statistical treatment in a relatively short time. The statistical value of such a series of judgments, however, is nil if we have no check upon the constancy of attitude under which they were taken.

Our *O*s found it possible to judge according to the three different instructions, but, except in a few cases, no one of these attitudes could be maintained constant for any large number of successive comparisons. It was even found that when one judgment only was required the stipulated basis of decision was very frequently supplemented by foreign criteria. Reid has suggested, with reference to his own instructions, that further refinement is desirable in order to eliminate equivocality. This may prove to be the case; but we are inclined strongly to the view that it is of far more importance that we secure a more thorough introspective check upon the comparison consciousness under the required attitudes. It may well be that we shall have to secure a report upon constancy of attitude after each comparison judgment. But we have found that even under such conditions there will be many judgments rendered under

contaminated instruction. We shall, therefore, be compelled to eliminate from statistical treatment all judgments made in a contaminated attitude. Under these conditions the labor involved in order to obtain sufficient judgments for statistical treatment will obviously be tremendous, but there seems no way out of it. It is obvious also that such a procedure narrows down work on the statistical sensory limen to that of the *S* highly trained in psychological introspection solely. There is one possible exception to this, on which, however, we do not have any data at the present moment. It may be that the *S* who is entirely naive to psychological introspection makes judgments under an uncontaminated stimulus attitude. But, so far as we can see, there is no possible way of ever determining this.

This much can be said, that even our highly trained psychological *S*s find it much easier to keep the attitude under stimulus instruction pure and uncontaminated than either of the process attitudes. And this may be because the *S* who is highly trained psychologically is after all a commonplace human being, who goes through life making many thousands of judgments under the commonplace stimulus attitude for every single judgment he makes under a strictly sensory attitude.

The failure judgments are without significance. The indices of significance of difference are greatest for the *O* with the lowest coefficients of precision and lowest for the *O* with the highest accuracy. The records of tendency of judgment are of no significance except that they are a check upon other statistical results.

Conclusions

(1) The effect of progressive practice is evident, with variations, under each of the three instructions, but it is most pronounced under the stimulus instruction.

(2) There is a high correlation between purity of attitude, as revealed by the introspective reports, and the indices of precision.

(3) The stimulus attitude yielded the highest precision of judgment.

(4) There is little relationship between carriage of instructions and the I. U.'s and the P. S. E.'s.

(5) Judgments on lifted weights under several instructions are of questionable value without an introspective check on every judgment.

(6) The three instructions used in this experiment, namely judgment on the basis of pressure on the finger-tips, judgment on the basis of kinaesthetic sensations in the wrist, and judgment on the basis of the stimulus itself, are susceptible of fulfillment for a trained *O*, but there can be no certainty of the constancy of the attitude over more than one judgment at a time.

THE MECHANISM OF CONSCIOUSNESS: PRE-SENSATION

By F. Russell Bichowsky, National Research Council, Washington, D. C.

This is the first of a series of papers describing systematically the mechanism of consciousness as known to introspection using modern methods. It will be convenient to begin with an investigation of the earliest effects of a stimulus on consciousness, and to treat the subsequent effects in an order that corresponds roughly with the classical division into sensation, percept, image and judgment. This paper deals only with the first of these divisions.

The first effect of a stimulus on consciousness, or at least on that part of consciousness which is susceptible to introspection, appears to be, not the full fledged classical sensation with its definite qualities of specific individuality, extension and duration, but something quite different.

Stimulus: a uniform patch of orange without visual boundary produced by alternating red and yellow illumination at a rate just sufficient to prevent flicker. *Instructions:* "Try to see the color 'red' in the color shown." *Preliminary period:* 30 sec. *O:* F. R. B. *Report:* "The (orange) color fairly leaped at me, that is to say, seemed to come accompanied with a feeling of shock. It was not localized at any particular distance and had no particular form. There was a decided kinaesthesis of drawing back. The color had no meaning. It was a feeling, pleasant and warm, though this was not the sensation 'warm' but the feeling. In the last experiment I attended to this warm feeling and had the sensation of bodily warmth which usually follows when anything is pleasant. This time there were the color feeling (not recognized as orange), the warm feeling (not recognized), and a pleasant feeling (this last intense, but not recognized). I can only talk about it because as I try to recall the state these pre-feelings are connected with the actual sensation. They did not do this during the experiment. Then they were timeless and did not seem to be located in space, at first. Then I was aware of a gradually developing strain *plus* a sort of drawing in of orange (entoptic) cloud figures, of which I was gradually becoming aware. These figures I did not interpret to be entoptic; they then seemed part of the feeling 'strain in the eyes' which was not fully interpreted as such. The strain-part of the total situation seemed to grow, the color and luminous aspect to decrease in amount. At this time I was beginning to feel the coming interpretation which was the kinaesthetic vocal image 'orange'; that is to say, I seemed to say 'orange' to myself. Up to this time the orange feel was non-focal, that is, was hardly 'seen' at all. On conclusion of the kinaesthetic vocal image [percept], the orange sensation was definitely located in space with a rough circular boundary formed of the orange-brown cloud-like figures. Suddenly kinaesthetic vocal strain intensified. The kinaesthetic vocal image 'red' developed without warning or other fore-phenomena. It did not possess meaning. There was rapid movement of the entoptic figures, quick pre-sensations from the disphragm, back of throat, and the chest, all of which ordinarily accompany the consciousness of mental shock. Here only the feeling, not the

recognition, was present. I can not remember all of the details, but I became aware of the *Aufgabe* as a kinaesthetic vocal strain as if trying to repeat the instructions. The kinaesthetic vocal image 'red' stood out particularly. The difficulty in remembering the details seems to have been due to the extremely rapid flow of content and the vividness of the following experiences. Movement of the entoptic border now became very rapid, though the color was less pronounced. Suddenly one fog-figure detached itself and became focal. At first it was purple gray, then reddish purple, then magenta. The figure now had a bright border. It was moving backwards. I was conscious of blinking my eyes and there was a strain in the eyes and throat. After winking the strain had disappeared, the orange background was now at a definite distance from me. It was not unspaced as it had first been. It was more reddish in tone than previously, the border being distinctly an unsaturated orange pink. There was unattended-to kinaesthetic vocal activity growing to a climactic 'It's red'. Reacted."

Though this report is my own, it is believed to be free from bias, being made before the theory of this paper was formulated. It is only slightly more detailed than the best obtained by my other *Os*. The introspections of all (31) *Os* apparently are consistent in all important points and lead to the following picture.

(1) The first conscious effect that can be traced to a stimulus of the sense-organs is a feeling which does not possess spatial or temporal quality, that is to say, is not felt to be located or extended in space or time, or to have the definite qualities and relations usually associated with sensations. Such feelings or pre-sensations, as they will be called, can not be described accurately, as they have none of the substantive or relational qualities necessary for description. They can only be felt.

Thus my *Os* report the first effect of a stimulus to be: "an undifferentiated something", "a feel", "without quality", "indescribable", "could not be attended to", "it just was", "it was not located in time", "in space".

(2) These pre-sensations, however, have emotional tone and feeling quality. They possess intensity. They differ with the kind of stimulus, but this difference is not describable except by incomplete figures of speech.

They were said to have "emotional tone". "They were an emotional tone spreading over the whole of consciousness." "It felt cold" [the pre-sensation blue]. "It was not, however, the cold 'feel' like that due to a cold stimulus." "The feels [of green and of the taste of cream] were both soothing and smooth and of about the same intensity but they were not alike."

(3) The pre-sensations tend to be followed by varying perceptual and imaginal contents which are distinct from them and which appear to be stimulated by them. At least, pre-sensation activity seems to be a necessary condition for percept activity.

"The 'feel' was unstable and tended to generate the sensation." "It acted as a stimulus." "It disappeared, the sensation took its place." "There was a closing in of the feel, followed by a sudden perception of position and shape. The feel still remained, however, as a background on which the new perception arose." "I can always feel the coming sensation.

Sometimes, though, I have the feel of a visual something without the perception of position or shape."

This stimulation of one bit of consciousness by others is not restricted to stimulation of percept by pre-sensation but appears to be a characteristic feature of many conscious transitions, especially of those where the transition is "logical" or predetermined.

In the particular case under investigation the stimulation of percept by pre-sensation may be likened to, if indeed it is not, the psychological correlate of the stimulation of a high level reflex arc by the activity of a lower one according to the familiar scheme of Hughlings Jackson and his school (Fig. 1*a*).

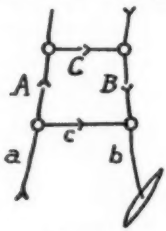


FIG. 1*a*

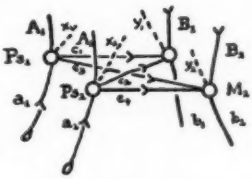


FIG. 1*b*

FIG. 1*a* The Hughlings Jackson scheme of reflex arcs
FIG. 1*b* The psychological relations of pre-sensations

(4) A given pre-sensation tends to stimulate a considerable range of percepts,—a perception pattern,—usually of its own modality. In this, which is not always the same for a given pre-sensation, there is usually a tolerably simple and constant part, the so-called sensation, which may be analyzed out of it, though this is an act involving more than direct perception. This sensation is almost invariably of the modality of the pre-sensation, but differs from it: (*a*) in that it follows a previous feel as if stimulated by it, and therefore has a history which the pre-sensation has not; (*b*)in that it is projected upon pre-sensation activity; (*c*) in that it has space and time relations (at least for sensations of most modes), such as size, shape; and (*d*) in that it has describable modality.

(5) A particular pre-sensation may fail, however, to stimulate its corresponding perception together with its subvariety sensation under a variety of circumstances. (*a*) When the particular sensation that would normally follow is inhibited by the "higher" activities of consciousness,—in a way that is reminiscent of the way in which activity of a high-level reflex arc of the Hughlings Jackson scheme inhibits the activity of a low level arc. (*b*) When for some reason some other percept activ-

ity is particularly susceptible to stimulation, so that the low-level stimulus drains off *via* the path leading to it and not *via* its normal path,—a circumstance which may be pictured, according to the Hughlings Jackson scheme, by assuming cross-paths (Fig. 1*b*) normally of high impedance leading to these centers.

Thus a pre-sensation may stimulate very different percepts from those that would be expected from the nature of the pre-sensation. The percept stimulated may even be of a different modality from the pre-sensation or the sensation normally stimulated by it; *e. g.*, the vocal "image" orange of the introspection quoted above.

This phenomenon, which does not appear to have been noted for a synaesthetic *S* is, as far as can be judged from data at hand, relatively common and forms a link between asynaesthesia and synaesthesia. This is important from the point of view of the student of synaesthesia, because it breaks down the supposedly sharp line between the synaesthetic and the asynaesthetic process, and because it gives strong confirmatory evidence for the view of Wheeler and Cutsforth[1] that synaesthesia is simply a form of perception, differing from ordinary perception only in that it is of a fixed modality regardless of the modality of the pre-sensation. This means, in anatomical terms, the existence of paths from pre-sensation arcs of one modality to percept-centers normally stimulated by arcs of another modality.

For the purposes of this theory, synaesthesia is important because the circumstance of a constant difference of modality between pre-sensation and perception makes it simpler to separate by introspection the "feel" of pre-sensation from the relations of the percept. Thus we can expect synaesthetic *Ss* to be most satisfactory for investigation of the nature of activity in the pre-sensation arc. Note, for instance, the descriptions of pre-sensation activity reported by Wheeler and Cutsforth[2]; "I find that I can attend to the sound as long as I do not attempt to analyze it; all the sound of which I am aware is an undifferentiated 'something'; it may be raw sound; it is meaningless; I interpret the experience as sound in terms of the vocal motor image 'sound' or my attention shifts to motor relaxation and I find myself merely responding to the sound situation. I simply can not focalize the sound as such in the absence of visual imagery."

(6) It is doubtful if a pre-sensation can be originated by any activity of the perceptional or other higher levels. Pre-sensations which apparently are originated in this manner (such as the red pre-sensation encountered above) are apparently always due to weak stimulation from the end-organs, such as entoptic phenomena. On this point data are not complete and further investigation is needed.

(7) When perception is inhibited by activity of higher arcs there seems to be no certain proof that the underlying pre-sensation is also inhibited. I originally thought that this customarily occurred, but greater practice in introspection has greatly decreased the number of cases where the pre-sensation

[1]R. H. Wheeler and T. D. Cutsforth, *Univ. of Oregon Publ.*, 1, 1922, No. 10.

[2]R. H. Wheeler and T. D. Cutsforth, *Psych. Rev.*, 29, 1922, 214. What is called a pre-sensation here is called by Wheeler and Cutsforth a primary sensation.

could not be detected even though perception or its subvariety sensation was inhibited. I now suspect that the residual cases where I have not been able to obtain direct introspective evidence of the existence of "sub-conscious" pre-sensations are simply due to lack of introspective technique.

Thus when the attention, that is the image-level, is engaged in seeing an apple, this activity of the higher level often completely inhibits the perception of extraneous smells and sounds of low intensity. Nevertheless, the existence of the corresponding pre-sensation of smells and sounds makes itself evident by the general affective tone. Thus the picture of an apple feels more pleasant in the presence of an unperceived fruity odor than in the presence of an unperceived putrid odor. The free association reaction is also markedly affected. Thus, to take an informal example, an unperceived spicy odor tends to suggest appropriate after-images associated with cooking, *e. g.*, pie, etc.

(8) When two or more end-organs are stimulated together so that two or more separate pre-sensations might be expected, apparently in every case fusion of some sort takes place, there being but one joint pre-sensation, not two separate ones. This is true even when the simultaneous stimuli are as diverse as, say, a visual stimulus in the median line and a tactile stimulus applied to the back of the hand. When two such incongruous stimuli occur the first effect on the consciousness is not that of two stimuli but that of a single fused pre-sensation partaking of the quality of both tactile and visual pre-sensation without spatial discrimination of its two components but with decided emotional tone, normally unpleasant, if the stimuli are moderately intense. This non-discriminating pre-sensation fusion usually, of course, almost immediately stimulates the appropriate separate percepts, or to be more exact, it stimulates the appropriate pattern, if such a pattern be possible, composed of the two appropriate sensations in the proper spatial relation to each other and projected on the original fused pre-sensation as on a background, a background which retains the emotional tone of the original fusion.

There seem to be no exceptions to this rule of pre-sensation fusion, though pre-sensations are so instable and such difficult objects for introspection that it is hard to be sure. At any event, cases which were once thought to be exceptional (the case used above to illustrate the rule is one) have either yielded to more careful introspection the distinct steps: (1) pre-sensation, (2) a percept-pattern involving both sensations in their appropriate relation; or they have yielded, in cases where the stimuli were even more incongruous, as in cases of binocular rivalry, an unpleasant pre-sensation having the quality of both stimuli, *plus* a percept involving only one of the two incongruous sensations, the other being suppressed temporarily at least. Still a third variety of reaction to incongruous pairs of stimuli appears to occur, namely, partial transfer of quality from the recessive to the dominant percept. Thus examples are reported in binocular vision where the color of an otherwise recessive figure is transferred to the dominant figure. These cases have not been carefully studied from the point of view of this paper.

(9) Pre-sensations may produce motor effects directly, either with or without conjoint stimulation of percept activity. This direct stimulation of motor reactions is, however, subject to inhibition by higher centers. For the higher classes of perception this inhibition is rather the rule. However, for some sorts of stimuli and under some conditions, the direct motor effect of a pre-sensation is more prominent than its effect as a stimulus for perception.

Thus, my *Os* found that an attempt to hold pre-sensations in consciousness by inhibiting percept-activity almost inevitably was partially frustrated by the direct stimulation by the unpleasant (under these conditions) pre-sensation of defensive motor activity of the semi-involuntary type. Thus, an attempt to hold a visual pre-sensation in consciousness almost invariably led to winking and to a consequent stimulation of new muscular and tactile pre-sensations due to that act.

In fact, though no very extensive study has been made, it is felt that winking, often thought to be an unconscious reflex in its normal functioning, is really stimulated by conscious, though involuntary pre-sensations, that is to say, by the "feel" of the visual stimulus. This is probably true for such other semi-involuntary reactions to strained conditions as the various motions in the throat and mouth tending to clear irritations, or as the suspension and intensification of breathing under emotional strain, or as various twitches of the lips, the nostrils and the body which aim to remove the cause of unpleasant cutaneous sensation,—in short many of the so-called "cortical" reflexes. This is also probably true for a whole series of semi-conscious pleasant sensations, *e. g.*, sexual reactions (a case which is complicated by control of even higher centers), the semi-involuntary reactions to the smell and the sight of food, the smile, and others.

The list given above is all that the author has personally investigated, but it is possible that it may be extended, perhaps, to cover the defensive and offensive reactions the back-stimulation from which is a large part of the sensation-content of emotion, as pointed out by the familiar James-Lange theory. It is not supposed that all emotions showing the James-Lange effects involve only direct reactions to pre-sensations; for there are cases where emotion demonstrably involves still higher centers. Restricted to cases where the emotion is felt before perception is involved, investigation shows that in the one case carefully studied here, namely, fear due to to a sudden intense stimulation, the whole conscious effect of the fear-producing stimulus is the conscious, unpleasant 'fear' pre-sensation *plus* the direct stimulation by that pre-sensation of the well-known semi-involuntary muscle reactions characterizing fear, *plus* the direct stimulation by these muscle reactions of the James-Lange pre-sensations.

However, regardless of whether the list of motor effects directly stimulated by pre-sensations can be extended to cover all of the motor reactions underlying the back-stimuli which form a large part of the total conscious situation in cases of emotion, the general connection between pre-sensation and a large number of direct defensive and offensive reactions is definite, and is entirely in accord with the analogy of the pre-sensation to the activity of a higher reflex arc of Hughlings Jackson's scheme.

The sense of pain is an especially good example not only of the direct stimulation of motor activity by pre-sensation activity but also of the general characteristics of pre-sensations. This is so presumably because of the high resistance of the paths from the pre-sensation to the percept cen-

ters. Whatever the reason, pain of low intensity, not accompanied by localizing tactile sensations of other modalities, is normally felt as pure pre-sensation, with but little accompanying perceptual activity. That is to say, pain is ordinarily just feeling without extension, position, substantial quality, or relations to other qualities. Nevertheless, or rather because of this fact, it is characterized, as the most elementary introspection will show, by a most pronounced tendency to stimulate defensive reactions, exactly as one would expect on the Hughlings Jackson scheme for an arc where, because of high resistance of the paths leading to and from the next higher arc, that higher arc cannot easily be stimulated by, or easily inhibit, activity of the lower arc.

In fact, it appears that there is no part of the psychological evidence given above which is inconsistent with the Hughlings Jackson model. Since this point is important, it will be wise to review the evidence.

It appears from the purely introspective data summarized in this paper that the relations between pre-sensations and other forms of conscious content, though of course not the qualitative nature of the pre-sensation as given in (1) and (2), can be summarized according to the diagram, Fig. 1*b*. According to this diagram, an impulse travelling up A_1 or A_2 produces at PS_1 or PS_2 a conscious effect, the pre-sensation. The nature of this conscious effect will differ with the kind of stimulus. In the case that two or more pre-sensations, say PS_1 and PS_2, are stimulated together, they simply sum up in consciousness without being discriminated; that is to say, they give a single feel, the resultant pre-sensation (PS_1-PS_2), not two separate feels (PS_1) and (PS_2). A single pre-sensation PS_1 may stimulate motor activity directly, as is indicated on the diagram by the paths c_1 and c_3 leading to the motor centers M_1 and M_2. This direct stimulation of motor activity may be inhibited by impulses originating at higher levels and travelling *via* B_1 and B_2. Also, because PS_2 may also send impulses to the same motor center *via* c_2, the motor reaction of M_1 will depend not alone on the activity of PS_1 and of higher centers, but also on the activity of PS_2, the resultant motor activity being not the sum of the activity that would be separately produced by PS_1 and PS_2, but possibly quite different, as for example when the activity stimulated *via* c_2 is such as to inhibit stimulation *via* c_1.

Besides stimulating motor activity directly, the pre-sensation may stimulate perception *via* the direct path A_1 or, under certain little understood conditions, *via* the high resistance path x_1. If it stimulates *via* A_1, the resultant percept will include what is ordinarily called the normal sensations appropriate to the path a_1A_1. If not, and if the stimulation travels *via* x_1, as it may in cases of synaesthesia, and in the cases described under (5), the product will be a percept which does not include the normal sensation and may even be of a different modality from it. There is, however, no opportunity, as is indicated by the

direction of the arrows, for percepts or higher activity either to stimulate or to inhibit pre-sensations. The pre-sensation is an entirely involuntary and automatic bit of consciousness.

As has been suggested, the diagram is similar to that which might be drawn by a physiologist of the Hughlings Jackson school to explain the physiological facts of the behaviour of the optic thalamus. So interpreted, our 'directions of stimulation' become conduction paths, our arrows unidirectional synaptic gaps, our pre-sensation centers neurones. It is, therefore, natural to assume that the facts of the connections of pre-sensations with other parts of consciousness, as shown on the diagram, are in fact imposed on pre-sensation by the physiological structure of the physiological acts involved, just as many of the psychological facts of vision are known to be conditioned by the physiological structure of the optic apparatus. If this assumption is made, it becomes immediately possible from a study of the effects of thalamic lesions to locate the organ of pre-sensation in the optic thalamus and analogous parts for conduction paths not involving the spinal cord.

Thalamic lesions of three types are known. (1) Those affecting the fillet and associated parts, in the present scheme the paths a_1 and a_2. With lesions of this type, both sensation and pre-sensation are presumably lost. (2) Those affecting the fillet-cortico-thalamus, which bundle presumably includes both the afferent and the efferent fibers A_1, A_2, B_1, B_2, x_1, x_2. Lesions in this area are followed by loss of sensation without loss of pre-sensation. (3) Those that affect the lateral zone of the thalamus without disturbing cortical connections. These lesions more or less interrupt the activity of the pre-sensation arc, especially the return paths B_1 B_2, which ordinarily serve to inhibit its excessive activity. As a result, in lesions of the type (2) and (3), where B_1 and B_2 are cut, the thalamus is freed from the normal cortical control and the patient exhibits the results of excessive thalamic activity, that is to say, the "syndrome thalamique" of Déjerine and his pupils,[3] characterized by spontaneous pains, curious crawling and tingling sensations of the unpleasant type, while sensations of the pleasant type have much greater affective quality. Thus, to quote Head[4]: "In one case, we were able to show that the patient could not recognize any thermal stimulation as such, and yet over the affected half of the chest large tubes containing water at from 38°C to 48°C evoked intense pleasure. This was shown not only by the expression of her face, but by her exclamations 'Oh, that's lovely, it's so soothing, so very pleasant'." Remembering the over reaction which is characteristic of the removal of higher control, these symptoms are what would be expected to follow lesion of the cortico-thalamic paths according to our theory. The only other type of symptom which might be expected from lesions in this area would be from those injuring the direct paths A_1 A_2, but leaving intact the oblique paths x_1 x_2. Anatomically speaking, this effect is probably impossible for gross injuries because of the close association of these paths in the post-thalamic fillet, but if possible the symptom expected would be synaesthesia. Relatively high resistance or low development of these direct paths would also

[3] J. Déjerine and M. Egger, *Rev. neurol.*, 13, 1903, 397: J. Déjerine and G. Roussy, *ibid.*, 14, 1906, 52; J. Déjerine, *Semiologie des affections du système nerveux*, 1914.

[4] H. Head, *Studies in Neurology*, 1, 383.

cause the same symptoms without the necessity of detectable lesions. This physiological theory seems sufficient to explain the phenomena of perceptual synaesthesia completely, and to be free from the objections urged by Wheeler and Cutsforth against previous physiological theories which postulate cross-connections from one percept-area to another with consequent transfer of functions, these objections being: the wide separation of the areas involved in perception; the lack of permanence of synaesthesia; the fact that one may have synaesthetic percepts of tones of a melody without having synaesthetic perception of melody; the high development of the auditory area necessary to account for the completeness of the correspondence of colored tones to stimuli, to take but one example, and yet the absence of the natural function of that area of perceiving the stimulus in its natural modality.

A SCALE FOR SCORING TESTS WITH ALTERNATIVE ANSWERS

By GILBERT J. RICH

Kohs[1] and Richards[2] have shown that the usual method of scoring tests in which the *S* checks one of two, three or four alternative answers to every item (by subtracting from the number of correct responses the number of errors, 1/2 the number of errors, or 1/3 the number of errors, respectively) does not eliminate the factor of chance as it is supposed to do, and permits the guesser to make a significant positive score. Richards, furthermore, suggests a new method of scoring tests of this type. In his procedure, the crude score is equal to the number of correct responses up to the point where the probability of obtaining the given number of correct answers by chance is .04. Beyond this point, the score for each successive number of correct items is obtained by adding to the preceding score the ratio of the probabilities of attaining by chance the two numbers of correct responses concerned.

Several points should be noted in connection with the method of scoring which Richards proposes. On the positive side, it is a definite effort to apply the calculus of probabilities to the problem of scoring and to weight the successive numbers of the test in inverse proportion to the probability of their being reached by guessing, that is, by chance. A performance that might easily be the result of a guess is of little weight; one into which the chance factor is not likely to enter receives a greater weight. In so far as it accomplishes this end to a greater degree than do the older methods, the new scale is a distinct advance. It also avoids negative scores, which are disadvantageous for practical purposes. On the other hand, the scale is mathematically discontinuous. The score is not a definite function of the number of correct answers. For low scores it is one type of function, for high scores an entirely different function. If it had been shown that the *S*'s performance in the test is actually best represented by two discrete curves, it might be proper to use this form of scale. But in the absence of such proof there is no justification for not using a single function. The use of two functions has the disadvantage of making high and low scores incomparable for statistical purposes, since they are derived from different bases. Moreover, the point at which the score changes

[1] S. C. Kohs, High Test Scores Attained by Subaverage Minds, *Psychol. Bull.*, 17, 1920, 1-5.

[2] O. W. Richards, High Test Scores Attained by Subaverage Minds, III, *J. Exper. Psychol.*, 7, 1924, 148-156.

from the one function to the other is purely arbitrary. There is no *a priori* reason for fixing upon a probability of .04 as the critical value.

It would seem possible, however, to construct a scale for scoring these tests which should retain the patent advantages of Richards' method and yet avoid the difficulty of a non-continuous scale. In such a scale it should be possible to express by means of a general formulation the score for any number of correct responses. The scale should: (1) increase from a maximum of zero for no items correct, thereby avoiding negative scores; (2) reach a maximum value of 100 when all items are right; (3) have successive steps which are inversely proportional to the probabilities of obtaining the respective values by mere chance (guessing); and (4) be mathematically homogeneous (expressible by a single general formula), so that all parts of the scale would be statistically comparable. Every one of these requirements may be met by merely carrying Richards' procedure to its logical conclusion. Instead of making the scale-increments inversely proportional to the probabilities only beyond an arbitrarily selected value, we may start with a zero score when all items are wrongly checked, and for the entire scale above zero form each successive value by adding to the preceding score an increment proportional to the ratio of the probabilities as mentioned above. The entire scale will then be homogeneous. A scale of this type is described below.

We may readily define our scale by stating the mathematical formula according to which any performance in the test is to be scored. We shall be dealing with a test of N items, each one of which is to be answered by checking one of M alternative responses (of which only one is correct and the others wrong). We desire the score when R items have been correctly checked. Let us call P_R the probability that exactly R items will be correctly answered purely by chance, T_R the score for R items correct upon an arbitrary scale, and S_R the same score linearly transformed to a scale of 100 units. Richards[3] has shown that:

$$P_R = \frac{N!}{R!\ (N-R)!} \times \frac{(M-1)^{N-R}}{M^N} \qquad (1)$$

If, now, each value of the scale, starting with zero, is formed by adding to the preceding value the ratio of the probability of obtaining by chance the preceding number of correct items to the probability of similarly obtaining the number of correct items in question, then:

$$T_R = 0 + \frac{P_0}{P_1} + \frac{P_1}{P_2} + \ldots\ldots + \frac{P_{R-1}}{P_R} \qquad (2)$$

[3] O. W. Richards, *op. cit.*, 149.

When R = o, none of the fractions can be set up, P_{-1} does not exist, and T_o = o. A final step is to transform the scores to a scale of 100. Since the maximum score is obtained when R = N, and this score is to be 100:

$$S_R = \frac{100}{T_N} \times T_R \qquad (3)$$

The value of S_R so obtained meets all the requirements set forth above. To illustrate the use of this scale, we have computed it for the same three examples for which Richards gives the values of his scale, namely; 25 items with 4 alternative responses, 25 items with 3 alternatives, and 50 items with two alternatives. The values of P_R for these three cases are given by Richards and Kohs.[4] As the practical use of a scale usually requires that it be limited to integral values, we have followed Richards' example and have rounded the figures by dropping all fractions in favor of the nearest integer. The completed scales are shown in the accompanying Table.

25 Items			*50 Items.*		*2 Responses*	
No. Right	Score 4 Resp.	Score 3 Resp.	No. Right	Score	No. Right	Score
25	100	100	50	100	25	5
24	66	67	49	72	24	5
23	50	50	48	58	23	4
22	40	40	47	49	22	4
21	32	33	46	43	21	4
20	27	27	45	38	20	3
19	23	23	44	34	19	3
18	19	19	43	30	18	3
17	16	16	42	27	17	2
16	13	13	41	25	16	2
15	11	11	40	22	15	2
14	9	9	39	20	14	1
13	7	8	38	18	13	1
12	6	6	37	17	12	1
11	5	5	36	15	11	1
10	4	4	35	14	10	1
9	3	3	34	13	9	1
8	2	3	33	12	8	0
7	2	2	32	11	7	0
6	1	2	31	10	6	0
5	1	1	30	9	5	0
4	1	1	29	8	4	0
3	0	0	28	7	3	0
2	0	0	27	7	2	0
1	0	0	26	6	1	0
0	0	0			0	0

An examination of any one of these scales will show that it accords but little value to the first few responses. This is as it

[4] O. W. Richards, *op. cit.*, Tables I and II; S. C. Kohs, *op. cit.*, Table I.

should be. The first few responses may be due to guessing. Indeed, it is improbable that a guess would not result in some correct items. As the number of correct responses increases, the value of each additional right answer becomes larger in proportion as the probability of its being attained by chance decreases.

The final test of this type of scale, as of any method of scoring, is of course its utility in producing results which are significant in interpreting the tests to which it is applied. We may here only suggest the mathematical basis of the scale; its practical value cannot be shown until it has been used and the results of its use evaluated. Thurstone[5] has proposed a method for computing the amount that each error should be penalized in any given test, but his method is applicable only to linear scoring formulae, and our formula is not linear. If the differences between the upper values of our scale are considered too large for educational or other applied use, the scale may be combined with some other scale in the way that Richards suggests, with any weighting desired. This, however, is a question into which we cannot enter, since the additional scale to be used and the relative weighting are arbitrary matters determined in large part by the particular problem at hand.

[5]L. L. Thurstone, A Scoring Method for Mental Tests, *Psychol. Bull.*, 16, 1919, 235-240.

THE DEFINITION AND MEASUREMENT OF ATTENTION

By H. M. JOHNSON, Mellon Institute, University of Pittsburgh

I

Contemporary psychologists make use of a single term, 'attention,' to designate two variables indifferently. One of these variables is the degree of consciousness or the clearness of content; the other is the degree of the sensorimotor adjustment of the organism with respect to a particular stimulus. Some authors appear to confuse the two concepts, others regard them as distinct and mutually supplementary. This paper examines them and suggests their mutual independence.

'Attention' as Vividness or Clearness

All of the influential non-behavioristic authors of the present day assert that the particular objects to which one 'attends' are perceived in higher degrees of vividness or clearness than the other objects which one cognizes at the same instant. Hence, the degree of attention which one pays to an object is capable of being indicated by the degree to which one is conscious of it; or, in other words, by the degree of clearness which the object, as an element of content, is observed to possess. The measurement of attention, in this sense, therefore becomes a problem of estimating, as well as one may, the degrees of clearness of each element of a simultaneous complex of content; or, figuratively, of 'distributing' the various elements according to the magnitudes of their clearnesses.

Experienced introspectors assert that the task of comparing the clearnesses of various portions of content, independently of their other characteristics, is not at all easy. The assignment of satisfactory numerical values to these clearnesses is still more difficult. After long practice, a few adepts in the art of introspection achieve at least a consistency in respect to the frequencies with which the several numerical values are recorded.

It is, of course, impossible for anyone but the introspector to make the observations; and some non-behaviorists have debated among themselves whether training actually increases one's acuteness and reliability in this sort of observation or, rather, tends merely to fix a conventionalized language-habit. Since such a question cannot be settled by means which the objective psychologist concedes to be valid, our attitude shall remain agnostic.

'Attention' as Adjustment for Reception and Response

The term 'attention' is also currently used to designate (1) the adjustment of the accessory sensory apparatus to facilitate optimal excitation of a specific group of receptors by a specific complex of stimuli; this adjustment being accompanied by (2) the adjustment of some specific effector-apparatus for appropriate response to those stimuli; together with (3) the inhibition of activities which might interfere with such reception and response; and (4) the innervation of a very diffuse system of musculature which plays no direct rôle in the adjustment, except, perhaps, by serving to drain neural current from inappropriate pathways, and to facilitate, by means of neural flux generated by such systematic activity, the maintenance of the adjustmental attitude.[1]

The details of this process may perhaps be made clearer if they are represented in a neural scheme which would be preposterous if taken too literally, but which possesses some pedagogical advantages. (Cf. Fig. 1.)

Let R_1 indicate a receptor connected by a chain of neurones to an appropriate effector M_1, the receptor being specifically irritable by a stimulus S_1, and being capable of adjustment as to position and direction by a muscle M_2, which may be considered as 'accessory' to R_1. If the stimulus S_1 be presented while R_1 occupies a position unfavorable to its reception (as in the position indicated by the dotted lines), the reaction-arc $R_1 M_1$ may be excited but feebly or not at all. Furthermore, if neural flux aroused in other receptors than R_1 is not being drained into M_1, the latter will possess little 'tonic' innervation; its sensitivity will be low, and S_1 will be subnormally effective in arousing its response.

Suppose, however, that a second receptor, such as R_2, be connected directly with the accessory muscle M_2, and less directly, *i. e.*, through a pathway of relatively higher resistance, with the muscle M_1. If the stimulus S_1 acts on receptor R_2, the result will be (*a*) contraction of M_2, bringing R_1 into a position favorable to stimulation by S_1; and (*b*) an increase in the irritability of muscle M_1, so that the arc R_1M_1, being reinforced by the arc R_2M_1, is now favorable to excitation by S_1.

Moreover, let us suppose that at some past time, a different stimulus, S_2, applied to the receptor R'_2, has been repeatedly presented shortly before, or simultaneously with, the first stimulus S_1. The result may have been the establishment of a conditioned response of M_2 and M_1, which is equivalent to that aroused by S_1 applied to R_2. In this respect the stimulus S_2 has become a substitute for S_1, or, in laboratory jargon, is a 'preparatory signal' for the latter, and has 'aroused attention' to it.

It often happens, however, that the stimulus S_2 is of momentary duration, while a considerable time elapses before S_1 is presented, but the organism, nevertheless, may hold its attentive attitude with respect to S_1

[1]As attentional adjustment to a particular object becomes habitual, factor (4) tends to become eliminated; so that the attentional attitude is assumed and maintained with greatly diminished 'strain.' This is especially noteworthy in the reaction-time experiment. The fact that the adjustmental process has been simplified by practice does not imply that it has been rendered less effective. The *S*, in this sense, is not less attentive when thoroughly trained than he was when unpractised; although after long practice, in *selective* reaction, he may assert that he reacts unconsciously.

during the period of delay. Now striped muscle cells, of which M_1 and M_2 were tacitly assumed to be composed, do not ordinarily remain in a state of contractile activity unless they receive continuous innervation. We must therefore assume that a second order of substitution of stimuli has occurred; that some stimulus, either external or internal to the organism, has been acting in the meantime. We might follow some of the recent authors of elementary texts and argue that this stimulus is 'mental' rather than physical; but that would not look well. The *deus ex machina* is not

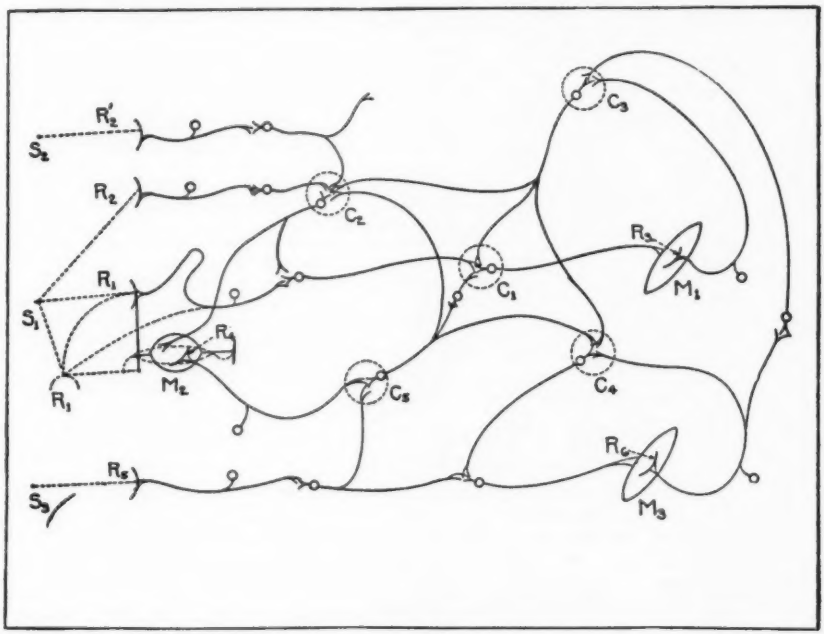


FIGURE I

to be utilized until the author has become helpless. We shall therefore seek the causal agent within the single system we have chosen. The activity of striped muscle is accompanied by chemical changes which constitute adequate stimuli for receptors in the muscle-spindles, and which may thus arouse arcs designated by Dunlap as 'homeodetic.' Let us designate such receptors in the figure as R_3 and R_4, and picture each of them as being connected with the contractile cylinders in M_1 and M_2. Once these muscles are excited, in whatsoever degree, the homeodetic arcs R_3M_1, R_3M_2, R_4M_1 and R_4M_2 may be thrown into circular activity. As long as their current is not diverted, the organism maintains its attitude of adjustment with respect to S_1; it is behaving as if it were being at least weakly stimulated by the latter.

To illustrate the fact of diffuse innervation, mentioned as factor (4) above, one need only multiply these homeodetic arcs. I shall therefore not complicate the drawing farther.

Let us now suppose that during this period of attentive delay, an irrelevant stimulus S_3 is presented to still another receptor R_3. Let us imagine R_3 as being strongly connected with the effector M_2, and also, but less strongly, connected with M_1; and let us connect the spindles R_3 and R_4 in M_1 and M_2 with M_3 also.

The flux from R_3 toward M_1 is therefore reinforced at the synapse C_5 by flux from R_4 and at C_1 by flux from R_3. If the arcs R_3M_1 and R_4M_1 are sufficiently dominant, the current from S_2 may be diverted into M_1 instead of passing to M_2. In such case, M_1 may be still further sensitized, and the pathway R_1M_1 may be rendered still more permeable to current excited by S_2 in R_1. The stimulus S_2 will therefore facilitate appropriate reaction (of M_1) to the stimulus S_1.

In case M_1 has been sufficiently sensitized by current from R_3 and R_4, the activity of the additional arc R_3M_1 may produce complete contraction, *i. e.*, an 'overt response' of M_1, the S behaving practically as if S_1 rather than S_2 had been applied. In such case he may be said to have been 'illuded' or 'hallucinated,' according as S_2 is external or internal to the body.

If, however, the irrelevant, or distracting, stimulus S_3 be sufficiently intense, extensive, enduring, novel, or the like, the arc R_3M_3 may drain the flux from R_3 and R_4 into the final common path from C_4 to M_3, and away from M_1 and M_2, thus diminishing or even abolishing the attentive attitude toward S_1. In such case, when S_1 is presented, the appropriate response of M_1 may be appreciably delayed, retarded or weakened: or it may even fail to occur in overt form.

The Relation between the Degree of 'Clearness' and the Degree of Adjustment

As I suggested at the outset, many authors, without defining the term 'attention' at all, include in their chapters on the subject a treatment of the two separate topics I have just sketched, as if the two somehow belonged together. One recent author asserts that he finds it extremely difficult to relate them; other authors avoid the difficulty by disregarding the question. However, two representative authors, namely, Münsterberg (*13*) and Dunlap (*6*, *7*, *8*), specifically postulate that the 'degree to which one is conscious' or the 'clearness and vividness' of content, is determined by the degree of adjustment for reception and response; the latter being perhaps theoretically expressible as some function of the number of afferent pathways which combine in producing the appropriate response. Certain other non-behaviorists are less specific in their formulations, but similar postulates seem to be implicit in their treatment.

A Crucial Experiment

Obviously the repertoire of a behaviorist, like the present writer, does not contain the acts which are appropriate to the problem. There is on record, however, an experiment by Cassel and Dallenbach (*2*), which to the present writer appears

to satisfy all the criteria which a non-behaviorist would regard as essential.

In the later and most representative part of this experiment, which alone I shall directly consider (since it was consistent with the earlier and rougher parts of the work), an experienced reactor, whose training in the introspection of attributive clearness had satisfied the standards of the department of psychology at Cornell University, was utilized in the following manner. He was required to react 700 times to an auditory stimulus (S_1), which was preceded by two presentations of a preparatory signal (S_2), given approximately 3 sec. and 1.8 sec.[2] before the appropriate stimulus (S_1). During a part or the whole of the preparatory interval an auditory distracting stimulus (S_3) was presented.—In the scheme given above, it will be seen that if the neural flux excited by S_2 were drained into the effectors M_2 and M_1, the effect would be to increase the *S*'s preparedness for the appropriate stimulus S_1, and to accelerate reaction to the latter. On the contrary, the distracting stimulus S_3 might at other times disturb the adjustment of M_1 and M_2 in at least two ways: either by draining from them the flux excited in the spindles R_3 and R_4 and thus diminishing their irritability; or by overstimulating M_1 so as to produce a premature response of at least gestural magnitude, and thereby necessitating readjustment to S_1 within a time which might be insufficient. In either case the distractor would retard reaction to S_1. From the account given in (*1*), a study to which this one was a corollary, it appears that sometimes one effect and sometimes the other was found, although the general tendency might be consistent throughout a daily series of reactions.

It should now be especially noted that if the magnitude of S_1 was sufficiently great not to tax the acuity of the receptors,—and the report does not suggest the contrary; and since, as we are informed in (*3*), the effects of fatigue and practice were rendered inconsiderable; *the reaction-time is determined chiefly, if not entirely, by the degree of adjustment for reception and response; and hence indicates the degree of 'attention' in the objective meaning of that term.*

Furthermore: after each reaction, while the necessary time (X) was being recorded by an *E*, the *S* recorded a number (Y) as designating the degree of clearness which characterized the noise of the sound-hammer as he perceived it.

Thus were obtained 700 pairs of simultaneous numbers, which were thenceforth capable of objective treatment.

Now, if the assumption is correct, that the degree of clearness is determined by the degree of preparation for reception and response, the coefficient of correlation, r_{xy}, between the gross measures of time (X) and clearness (Y), would approximate to unity provided the relationship be rectilinear; and the correlation-ratio, at least, should approximate to unity, though the relationship be less simple.

In fact, however, no such tendency toward association occurred. The results of such treatment, if resorted to, were not published by the authors: but the present writer, in a general review (*11*), showed, by an indirect and approximate method (the data not having been presented in a form which

---

[2] "The first flash, the 'ready' signal, was *set* to occur 3 sec., and the second, the 'now' signal, 1½ sec. before the occurrence of the stimulus. *As timed by the Hipp chronoscope* they occurred $3265 \pm 32.6\sigma$ and $1853 \pm 11.8\sigma$ respectively before the stimulus" (*1*, 130 f.). Italics are mine. In (*3*) it is alleged that I committed an inaccuracy in using the above values. My review does contain a copyist's substitution of 'Geissler tube' for 'a 2.9 volt electric globe (*sic*).' For this I tender my regrets.

permitted of direct computation) that the correlation r_{xy} was of the order of -0.25,[3] subject to a probable error of about 0.025. With this finding one of the original authors has since expressed himself (*3*) as being "in substantial agreement."

The import of this result is critical. While a correlation which is ten times its probable error is not to be attributed to chance, its significance should be very clear. The quadratic mean of the deviations of the empirical values of Y from the values calculated from the regression-equation is $\sigma_y \sqrt{1-r^2_{xy,}}$ in which σ_y is the quadratic mean of the deviations of the empirical values of Y from their mean M_y. Since in this case $r_{xy} = -0.25$, and $\sqrt{1-r^2_{xy}} = 0.97$, it follows that the precision with which a particular value of clearness, Y, can be inferred from the corresponding reaction-time, X, is but 3% greater than the precision based on a mere guess from the average M_y. The increase in precision is of the order of magnitude of the probable error of the index of precision itself, and is practically negligible. Furthermore: as I shall show in the second part of this paper, the precision with which the most representative value of a group of clearnesses taken at random can be inferred from the *mean* of their corresponding times is likewise 3% greater than the precision which attends a guess from the general average of clearnesses M_y.

These facts alone should suffice to settle the question. Under the conditions of this experiment the degree of clearness, as indicated by the introspector's numerical values, and the degree of preparedness for reception and response, as indicated by the reaction-time, are *practically* independent variables. The unknown value of neither can be inferred from the known value of the other with a precision which is significantly greater than the precision which attends the best guess that can be made without knowledge of the relationship.

The results, as far as they go, suggest that the degree of clearness and the degree of sensori-motor adjustment are practically unrelated: certainly neither a particular reaction-time nor the mean of a fractional group of reaction-times taken at random yields 'an objective measure of attributive clearness.' At the least, they are sufficient to negate the general proposition that clearness or vividness is determined by the degree of integration of the several reactive mechanisms in the execution of a specific response. That proposition can hereafter be reiterated as general only by an overt or implied "challenge" of the concreteness or of the appropriateness of designation of one or both sets of the correlated numbers. Toward such procedure the present writer has never been seriously inclined.

In the light of the facts, I see no reason why a description of attentive behavior, made in strictly objective terms, need be accompanied by a discussion of clearness of content, to complete the exposition of facts relevant to the former topic.

The term 'attention' might be used to characterize either set of events, but surely not both, unless these results are to be discounted. The second, or objective, meaning of the term

[3]Two methods of approximation were in fact used. The one outlined in (*11*) yielded a value of -0.24; the one suggested on p. 612 of this paper yielded -0.26. The mean value of -0.25 was considered sufficiently accurate for the purpose for which it was to be employed.

is more nearly in accordance with its use in every-day speech; the first in accordance with the literary fashions in psychology which have prevailed during the last fifty years.

II

Enough has been said about Cassel and Dallenbach's experiment to show that, however subsidiary they may have regarded it, the results have capital importance in the development of a scientific system of psychology. The problem is fundamental; the experimental procedure unimpeachable; and the qualifications of the reactor accepted by the school which sponsors the method. Since a similar experiment is unlikely to be made in a more favorable combination of circumstances, it is desirable that the exhibition of the results be freed from certain ambiguities that still remain.

The original report contains no intimation of the trend of the results which I have pointed out and which one of the authors has now admitted. On the contrary, that report, and also Dallenbach's comment (*3*) on my review (*11*), contain assertions which have been presented by certain lecturers as indicating a perfectly effective tendency in the opposite direction. The printed discussion to which I have referred, together with some private correspondence with Dr. Dallenbach, raises certain doubts in my mind whether this interpretation on the part of my colleagues correctly represented the authors' views, but in any case it is erroneous. I shall therefore examine the assertions which produced this impression.

Having obtained the 700 simultaneous numbers designated as gross measures of time (X) and clearness (Y), the authors treated them in a most unusual manner. The most obvious first step would have been to find the coefficient of correlation between the gross measures or, in its stead, the correlation-ratio. This procedure they rejected as being of no use for their purpose. According to the description in (*3*), that purpose seems to have been to ascertain how strong the tendency toward association would have been if no counter-tendencies had existed. At any rate, they proceeded as follows.

First, they distributed the reaction-times according to the class-values of the clearnesses associated with them. These classes were made large in extent and few in number, so that 96% of the total number of measurements fell within three contiguous classes, the middle one of which contained the mean, and 62% of the total number. Next, they averaged the measurements of times as thus distributed; and finally, they correlated the *averages* of the *distributed* times with the values of clearness which had formed the basis of the distribution. This procedure accomplished two fictitious results: first, it eliminated the variability of the clearnesses associated with the several times; and, secondly, it eliminated the variations among the times associated with each particular class of clearness. The correlation thus obtained, of course, approximated very closely to unity; but, nevertheless, it does not afford a means of deducing any *unknown* value of clearness—central or particular—from the corresponding value of time. The values of clearness must be directly obtained before the distribution of times can be effected; in other words, before one can make the inference, one must possess the facts to be inferred.

This treatment of the results, by suppressing whatever tendencies toward independent variability may have existed, imposed upon the data all the characteristics of a perfect correlation which they lacked. The computation which followed merely exhibited those characteristics, with-

out discriminating between those which were inherent and those which were imposed. The resulting coefficient reveals little more than the fact that no large and uncompensated errors were made in the arithmetic.[4] The authors, however, make the following assertions.

(1) "The present experiment shows that reaction-time is closely correlated with clearness" (2, 207, footnote).

(2) "We may conclude at any rate that, under these conditions of training, attributive clearness may be measured by the average duration and the mean variation of the simple sensory reaction" (207).

(3) "These data[5] show that *the rate of a simple sensory reaction* and its degree of precision as expressed by the m. v. are both (under our conditions) reliable means of determining the degree of clearness. It therefore follows, since attention itself is measurable in terms of clearness, that these objective measurements also give us a reliable index of attention" (206. Italics are mine).

In the opinion of the several professional mathematicians who have favored me with a critical examination of the foregoing analysis and of my former review, the argument which I have just given is sufficient to make it clear that the propositions in the first and second quotations, and the first proposition in the third, are not valid inferences from the results of the author's statistical treatment. Furthermore, the insignificantly small value (−0.25) of the product-moment coefficient of correlation between the gross measures of time and clearness plainly contradicts the authors' assertion that "reaction-time is closely correlated with clearness," and its corollary, that attributive clearness may be reliably inferred from "the rate of a simple sensory reaction."

Dallenbach, in his remarks (3) on my review, has now abandoned these propositions, intimating that they were misconstrued; but he reiterates that "attributive clearness may be measured by the *average* duration..... of the simple sensory reaction" (307). While conceding that my argument is valid, he dismisses it as 'irrelevant' to the latter proposition.

It now becomes necessary to clarify this proposition also, since it tends to obscure the important finding that these two variables— specific adjustment and clearness of content—are practically unrelated.

We must first ascertain the meaning of the expression 'average duration' or 'average reaction-time.' Does it refer (1) specifically to the means of times which were associated with specific class-values of clearness; or does it refer (2) to the averages of times taken without regard to the numerical values of clearness associated with them? Since no restrictive expressions are used, the reader is likely to assume the second meaning. Several readers have done so, I being among their number. However, I now suspect that the restricted meaning was intended, although the authors omitted to indicate the fact when its mention might have abbreviated the discussion. I shall now consider both alternatives.

If the expression be taken in the restricted sense, the assertion just quoted is certainly true. It is, however, subject to the following objectionable features. First, it is without practical significance, since, as I have pointed out, the numerics designated as 'clearness' must be given directly before they can be inferred from the averages of the distributed times. Secondly, the assertion is misleading, since the restrictions on the term

[4] A similar procedure, except as to the grouping in classes, was employed by Dallenbach in (4). I am informed that it appears sporadically in the field of educational statistics; but I know of no other author who has used it more than once.

[5] *I. e.*, those of the earlier part of the work, shown in their Table 1, which the later results "corroborate" (207).

'average duration' are not denoted, and since such expressions are so habitually used by readers in the unrestricted sense that their reaction-habits are not likely to be disrupted by the work they are likely to expend on a single paper. And, thirdly, the assertion, taken literally, is irrelevant to a number of passages in the article with which it was connected. Some of these passages are quoted above.

On the other hand, if the term 'average duration' be taken in the unrestricted sense, the assertion quoted is certainly false, for the data from which it was derived actually prove the contrary. This disproof is implied in the fact that the correlation between the gross measures is insignificantly small. I have not seen, thus far, a discussion of the relationship between the coefficient of correlation, r_{xy}, between the gross measures of X and Y and the coefficient of correlation, $r_{m_x m_y}$, between fractional means of simultaneous values of X and Y taken at random. I shall therefore give, without claim to priority, an original proof that $r_{xy} = r_{m_x m_y}$, which should finally dispose of the matter.

Definitions

(1) v, any particular gross measurement made on a variable quantity, such as X or Y.

(2) N, the total number of such measurements, taken at random. In the reasoning which follows N is assumed to be large.

(3) $M = (1/N)\Sigma_o^N v$, the general empirical mean, which, from the value of N, and the manner of sampling, is assumed to be practically equivalent to the 'ideal' mean.[6]

(4) f, the total number of groups in which the N measurements may be fractionated.

(5) n, the number of measurements included in any fractional group.[7]

(6) $m = (1/n)\Sigma_o^n v$, the mean of the measurements included in any fractional group.

(7) $\Delta = v - m$, the deviation of any measurement from the fractional mean.

(8) $d = v - M = (v - m) + (m - M) = \Delta + (m - M)$, the deviation of any particular measurement from the general empirical mean.

(9) $s = \sqrt{(1/n)\Sigma_o^n \Delta^2}$, standard deviation, or quadratic mean deviation, of particular measurements from the fractional mean, m.

(10) $\sigma = \sqrt{(1/N)\Sigma_o^N d^2}$, standard deviation of particular measurements from the general empirical mean, M.

(11) $\sigma_m = \sqrt{(1/f)\Sigma_o^f (m - M)^2}$, standard deviation of fractional means from the general empirical mean.

---

[6] It should be noted that the restrictions on definitions (2) and (3) are implied in most applications of the rules of probability to empirical data.

[7] In the treatment which follows it is assumed that the several simultaneous values of n are equal to each other. This method of fractionation is unnecessary to the proof. If a different method be used the relations here exhibited would still hold, provided the proper weighting factors were employed where necessary.

(12) $\sigma_m = s \div \sqrt{n-1}$, the standard deviation of the fractional means from the ideal mean; the latter, by restriction on definition (3), being adequately approximated by M, this formula gives the most probable value of σ_m by definition (11), if the empirical value of the latter be unknown.

(13) $r_{xy} = \Sigma_0^N (X - M_x)(Y - M_y)/N\sigma_x\sigma_y$, the product-moment coefficient of correlation between simultaneous particular gross measurements of X and Y.

(14) $r_{m_x m_y} = \Sigma_0^f (m_x - M_x)(m_y - M_y)/f\sigma_m\sigma_m$, the product-moment coefficient of correlation between fractional means of simultaneous gross measurements of X and Y taken at random.

To prove that $r_{m_x m_y} = r_{xy}$

In general, if a particular value of Y be not empirically known, its most probable value, Y, may be deduced from the empirical value of X which corresponds to it, according to the regression-equation

$$Y = M_y + r_{xy}\frac{\sigma_y}{\sigma_x}(X - M_x) \qquad (I)$$

If equation (I) be used in the estimation of N values of Y, on the assumption that $Y = Y$, the resulting errors will be $Y_1 - Y_1$, $Y_2 - Y_2$, ... $Y_N - Y_N$; and the standard error of estimation will be

$$\sqrt{(1/N)\Sigma_0^N (Y - Y)^2}$$

which, according to Yule (*21*), Kelley (*15*) and others, is equal to $\sigma_y\sqrt{1 - r^2_{xy}}$. The factor $\sqrt{1 - r^2_{xy}}$ is sometimes called the "coefficient of alienation."

In the special case in which $r_{xy} = 0$, or in which its sign as well as its value is unknown, the second term in the right member of (1) cannot be used; in which case (1) becomes $Y = M_y$, which is the best guess that can be made in the circumstances, and which is subject to a standard error equal to σ_y.

If the coefficient of alienation be not considerably less than unity, the use of X as a means of estimating the corresponding value of Y from the regression-equation does not appreciably increase the precision of estimation over the precision which attends the bare guess that $Y = M_y$. In such case the two variables X and Y may be said to be *practically* uncorrelated; for regardless of the reliability with which the value of the coefficient of correlation has been determined, the latter is too low to have practical significance.

From equation (I),

$$Y_1 = M_y + r_{xy}(\sigma_y/\sigma_x)(X_1 - M_x),$$
$$Y_2 = M_y + r_{xy}(\sigma_y/\sigma_x)(X_2 - M_x), \ldots\ldots\ldots\ldots \text{and}$$
$$Y_n = M_y + r_{xy}(\sigma_y/\sigma_x)(X_n - M_x)$$

By definition (6), the result of summation and division may be written

$$m_y = M_y + r_{xy}(\sigma_y/\sigma_x)(m_x - M_x)$$

On the assumption that $m_y = m_y$, the last equation becomes

$$m_y = M_y + r_{xy}(\sigma_y/\sigma_x)(m_x - M_x) \qquad (II)$$

The error implied in this assumption is equal to $m_y - m_y$; hence, if equation (II) be used to estimate f values of m_y, the standard error of estimation will be

$$\sqrt{(1/f)\,\Sigma_0^f\,(m_y - m_y)^2}$$

From fundamental principles in correlation it is evident that equation (II) may be written

$$m_y = M_y + r_{m_x m_y}\left(\sigma_{m_x}/\sigma_{m_y}\right)(m_x - M_x) \qquad \text{(III)}$$

whence the standard error of estimation of m_y is $\sigma_{m_y}\sqrt{1 - r^2_{m_x m_y}}$.

Inspection of equations (II) and (III) shows that

$$r_{xy}\,\sigma_y/\sigma_x = r_{m_x m_y}\,\sigma_{m_x}/\sigma_{m_y}$$

from which it is obvious that

$$r_{xy} = r_{m_x m_y}$$

in case $\qquad \sigma_{m_y} : \sigma_y = \sigma_{m_x} : \sigma_x,$

which remains to be shown.

By definition (8),

$$d^2 = \Delta^2 + 2\,(m - M)\,\Delta + (m - M)^2$$

and by summation

$$\Sigma_0^n d^2 = \Sigma_0^n \Delta^2 + 2\,(m - M)\,\Sigma_0^n \Delta + n\,(m - M)^2$$

The second term in the right member being equal to zero, division by n yields an expression which by definition (9) may be written

$$(1/n)\Sigma_0^n d^2 = s^2 + (m - M)^2$$

By summation between o and f, the last expression gives

$$(1/n)\Sigma_0^f\,\Sigma_0^n d^2 = \Sigma_0^f s^2 + \Sigma_0^f (m - M)^2$$

which upon division by f becomes

$$(1/N)\,\Sigma_0^N d^2 = (1/f)\,\Sigma_0^f s^2 + (1/f)\,\Sigma_0^f (m - M)^2$$

By definition (10) the left member is σ^2, and by definition (11) the second term of the right member is σ^2_m; hence the last equation becomes

$$\sigma^2 = (1/f)\,\Sigma_0^f s^2 + \sigma^2_m \qquad \text{(IV)}$$

By definition (12), and subject to the restriction on definition (3), if any empirical value of s be unknown its most probable value is $\sigma_m\sqrt{n-1}$; hence,

$$s^2_1 = (n - 1)\,\sigma^2_m,$$
$$s^2_2 = (n - 1)\,\sigma^2_m, \ldots \quad \text{and}$$
$$s^2_f = (n - 1)\,\sigma^2_m$$

Summation and division yield

$$(1/f)\,\Sigma_0^f s^2 = (n - 1)\,\sigma^2_m$$

By substitution and collection in (IV)

$$\sigma^2 = n\,.\,\sigma^2_m$$

whence $\qquad \sigma^2_m = \sigma^2/n$

and $\qquad \sigma_m = \sigma/\sqrt{n}$

Reverting to equations (II) and (III), one may note that since $\sigma_{m_y} = \sigma_y / \sqrt{n_y}$ and $\sigma_{m_x} = \sigma_x / \sqrt{n_x}$ and since, m_x and m_y being means of simultaneous numbers, $n_x = n_y$,

$$\sigma_{m_y} : \sigma_y = \sigma_{m_x} : \sigma_x$$

whence

$$r_{m_x m_y} = r_{xy} \qquad \text{Q. E. D.}$$

Geometrically the facts may be represented thus: if the several values of X be plotted against the corresponding values of Y, and the several values of m_x be plotted against the corresponding values of m_y. the two systems of points will lie within two similar ellipses, whose corresponding axes bear the ratios of $\sqrt{n} : 1$. The effect of correlating fractional means of gross measurements taken at random, instead of the gross measurements themselves, is the same as the effect of grouping the data into classes, by multiplying the two units of measurement by a constant factor. In either case, the resulting coefficient of correlation is the same as the correlation between the gross measures.

It follows from the above exposition that the two coefficients of alienation $\sqrt{1 - r^2_{xy}}$ and $\sqrt{1 - r^2_{m_x m_y}}$ are also equal. Since in the experiment of Cassel and Dallenbach $r_{m_x m_y} = r_{xy} = -0.25$, approximately, the fractional mean m_y of a group of numbers taken at random, designating the reactor's estimates of the clearness of his content, can be deduced from the mean m_x of the corresponding reaction times, subject to a standard error of estimation which is 0.97 times as large as the error attending a bare guess from the average.[8]

The conclusion is inescapable. Under the conditions of the experiment the tendency of the larger numerical values of 'clearness' to be associated with the shorter reaction-times was so weak that knowledge of a particular reaction-time, or of the mean of a number of reaction-times, does not enable prediction, with tolerable accuracy, of the unknown clearnesses[9] asso-

[8]The standard deviation of the empirically obtained times (X) in Cassel and Dallenbach's Table 2, from the values calculated from the regression-equation, is approximately 0.965 times as large as the standard deviation from their general empirical average. This value agrees well with the theoretical value of 0.97. In making the calculation I assumed that each of the standard deviations from the partial means given in the table is 1.25 times the mean variation from those means, as in a normal distribution.

[9]The term "clearnesses" is used intentionally. If the averages of the distributed times in the authors' Table 2 represent distributions which are approximately normal, then from Chauvenet's criterion we should expect the following. In the highest class of clearnesses reported, *i. e.*, "95," are some measurements associated with times longer than the time associated with the lowest degree, "75." In the class of clearnesses designated as "80", which is the lowest class containing more than one measurement,

ciated with them. The latter have to be ascertained by direct methods.[10].

Quotations 2 and 3, p. 608 of this paper, and the portion which I omitted from the quotation from 3, 307, assert that the degree of clearness can be reliably inferred from the mean variations from the partial averages of times. This point may be disposed of without prolonging the discussion. If averages of distributed times are meant, the objections cited on pp. 608f of this paper apply. If times, whether distributed or undistributed, are referred to, the assertion is an inconsequence of the data exhibited.

References

(1) E. E. Cassel, and K. M. Dallenbach, The effect of auditory distraction upon the sensory reaction, this JOURNAL, 29, 1918, 129-143.

(2) E. E. Cassel and K. M. Dallenbach, An objective measure of sensory clearness, *ibid.*, 205-207.

(3) K. M. Dallenbach, Dr. Johnson on reaction-time experiments, *ibid.*, 35, 1924, 305-307.

(4) K. M. Dallenbach, The measurement of attention, *ibid.*, 24, 1913, 465-507.

(5) D. Jackson, The algebra of correlation, *Am. Math. Mo.*, 31, 1924, 110-121.

(6) K. Dunlap, *A System of Psychology*, 1910.

(7) K. Dunlap, *Mysticism, Freudianism and Scientific Psychology*, 1921.

(8) K. Dunlap, *Elements of Scientific Psychology*, 1922.

(9) L. R. Geissler, The measurement of attention, this JOURNAL, 20, 1909, 473-529.

---

are some measurements paired with times longer than the average time associated with the highest class of clearnesses, "95." The times within a range of 0.064 sec. are associated with every grade of clearness reported. Obviously, then, the most representative value of clearness, Y, associated with a particular value of time, X, and *a fortiori* with n times whose mean is m_x, is m_y.

It is admitted that these ranges, as well as other characteristic features of the assembled data, have been estimated from parameters computed by approximate methods, which are strictly applicable only to normal distributions. But the empirical data, in the form of a scatter-table or scatter-graph, were not made available—a fact that has caused me much regret.

[10]Even the distributed means of times, despite their perfect correlation with clearness, lack in some cases the indicative value which the authors claimed for them. The highest correlation reported was for the results obtained with the tuning-fork as distractor, in Table 1. Its value is given as [−] 0.999. The two highest classes of clearness contain 98% of the measurements. The difference between the distributed averages of times is but 1.3 times its probable error, and in absolute units is but 0.0013 sec. This difference is smaller than the limit of accuracy within which the Hipp chronoscope can *measure* the time required for a free body to fall from a constant height.

In the procedure used to get these correlations there are some peculiarities which are interesting though not relevant to the main question, since the procedure itself is fundamentally invalid. The Pearson formula was applied to distributions which contained too few classes to render it applicable. (In the instance cited but two classes were used.) This fact by itself insures a large coefficient. In some instances classes which deviated widely from the central class were eliminated because of the small number of measurements which lay within them.

(10) D. Jackson, The algebra of correlation, *Am. Math. Mo.*, 31, 1924, 110-121.

(11) H. M. Johnson, Reaction-time measurements, *Psy. Bull.*, 20, 1923, 562-589.

(12) H. M. Johnson, Speed, accuracy and constancy of response to visual stimuli as related to the distribution of brightnesses over the visual field, *J. Exp. Psy.*, 7, 1924, 1-44.

(13) H. Münsterberg, *Grundzüge der Psychologie*, 1900.

(14) W. B. Pillsbury, *Attention*, 1908.

(15) T. L. Kelley, *Statistical Methods.*

(16) E. B. Titchener, *The Psychology of Feeling and Attention*, 1908.

(17) H. Woodrow, The measurement of attention, *Psy. Monogr.*, 17, 1914, No. 1.

(18) H. Woodrow, Outline as a condition of attention, *J. Exp. Psy.*, 1, 1916, 23-29.

(19) H. Woodrow, The faculty of attention, *J. Exp. Psy.*, 1, 1916, 285-318

(20) W. Wundt, *Physiologische Psychologie*, iii., 1903.

(21) G. U. Yule, *An Introduction to the Theory of Statistics*, 3d ed.

THE MEASURE OF APPROXIMATION OF DATA TO THE PHI-GAMMA HYPOTHESIS

By GILBERT J. RICH

Urban[1] recently called attention to observations by Hoisington and by Thomson which indicate that the agreement between the observed values in a psychophysical experiment and the values computed according to the *phi-gamma* hypothesis tends to increase with practice, and suggested that the relationship could be worked out in detail from existing data. Fernberger,[2] acting upon this suggestion, computed the sums of the squares of the differences between observed and calculated values for two sets of experiments upon lifted weights. It should be noted, however, that the conclusions of Hoisington and of Thomson were arrived at by applying different criteria of approximation, and that Fernberger used in his calculations only the method of computation employed by Hoisington, not the more adequate method of Thomson.

Hoisington[3] calculated the sums of the squares of the deviations of the percentages obtained experimentally from the theoretical percentages obtained according to the *phi-gamma* hypothesis. Properly to understand the differences between the two methods of measuring agreement, several points with respect to this criterion must be noted. (1) The raw or unweighted squares of the deviations are used in the computation. (2) The sum of the squares is a measure of *badness* of fit of the theoretical curve to the data, since a low value indicates a close approximation and a high value a poor approximation. (3) There is no standard of reference when this criterion is used. The final values obtained range from zero (perfect agreement) upward to a possible maximum of as many units as there are comparison stimuli. The values for different experiments are comparable only in case the number of comparison stimuli is the same (although they may be made comparable by division in each case by two less than the number of stimuli, as in the work of Williams[4]). (4) Finally, the number of observations made does not enter into the calculation, so that the resulting

[1]F. M. Urban, The Approximation of Actual Data to the Phi-Gamma Hypothesis, this JOURNAL, 34, 1923, 496-497.

[2]S. W. Fernberger, The Approximation of Actual Data to the Phi-Gamma Hypothesis, *ibid.*, 34, 1923, 498-500.

[3]L. B. Hoisington, An Example of the Fractionation of Data from the Method of Constant Stimuli for the Two-Point Limen, *ibid.*, 28, 1917, 588-596.

value becomes no more delicate as an indication of agreement as the number of experiments increases.

Thomson's[5] conclusions as to the effect of practice were arrived at by means of the application of his adaptation of Pearson's method for testing for goodness of fit[6]. This differs from the older and simpler method (of Urban) in every one of the respects noted. (1) The bases of computation are the weighted instead of the unweighted squares of the deviations between the experimental and the theoretical percentages. This difference is of utmost importance, for it alone is responsible for the divergent results obtained by the two procedures. (2) The final value obtained, P, is not itself the sum of the squares, but an inverse function (ogival in character) of that sum. Thus, P is a measure of *goodness* of fit. It is high when the approximation is close and low when it is poor. (3) P is not a random value but is a percentage probability, varying from a possible minimum of zero to a maximum of 100% (perfect agreement). It measures the probability of obtaining in a random sample of the size taken (number of observations) a curve as bad as or worse fitting than that actually determined by experimentation, if the hypothesis used were true. (4) The sum of the squares, before being used in the calculation of P, is multiplied by the number of observations made at each comparison stimulus. This gives a final value that is dependent upon the extent of the experiment, becoming a more delicate test as the number of experimental series increases. That it should do so is inherent in the definition of P as just given, which takes into consideration the size of the sample.

We cannot enter here into a consideration of the relative merits of the two criteria of approximation. Thomson[7] has fully treated the underlying mathematics and, it would seem, has demonstrated that his method is the more logical of the two and the more adequate for scientific purposes. Out interest lies in the fact that they are essentially different. We should expect, if they agreed, to find that, in comparing closeness of approximation of two sets of data, wherever the figure obtained by Urban's method (the method used by Hoisington and Fernberger) was the larger for one set, the value of P according to Thomson's method would be the smaller for that same curve.

---

[4]H. D. Williams, On the Calculation of an Associative Limen, *ibid.*, 29, 1918, 222.

[5]W. Brown and G. H. Thomson, *The Essentials of Mental Measurement*, 1921, 90ff.

[6]K. Pearson, *Tables for Statisticians and Biometricians*, 1914, xxxiff.; G. H. Thomson, The Criterion of Goodness of Fit of Psychometric Curves, *Biometrika*, 12, 1919, 216-230; W. Brown and G. H. Thomson, *op. cit.*, 77ff.

[7]G. H. Thomson, *loc. cit.*

As will be shown below, this is by no means always the case. The reason for the discrepancy is to be found in the difference between the use of unweighted and of weighted squares of the deviations. None of the other differences between the two methods would account for inversions in cases where the number of observations does not differ.

To apply the more rigorous criterion of goodness of fit to the present problem of the effect of practice upon the approximation of data to the *phi-gamma* hypothesis, we have computed the Pearson-Thomson P for the same experimental data for which Fernberger calculated the raw sums of the squares.[8] The resulting values are given in Tables 1 and 2, which are numbered to correspond with the Tables in Fernberger's paper (corresponding to Urban's Table 18 and Table 19, respectively).

TABLE 1

Group Number	Lighter Judgments	Heavier Judgments
10	.953	.667
11	.963	.870
12	.835	.968
13	.326	.840
14	.399	.292
15	.897	.557
16	.065	.685
17	.443	.968
18	.423	.570
19	.592	.270
20	.561	.435
21	.672	.036
22	.498	.353
23	.670	.113
24	.926	.244
25	.003	.820
26	.945	.291
27	.995	.985
28	.520	.990
29	.948	.835
30	.876	.998
31	.487	.350
32	.738	.993
33	.785	.023
34	.707	.622
35	.747	.630
36	.493	.529
37	.627	.072

TABLE 2

Group Number	Lighter Judgments	Heavier Judgments
1	.349	.846
2	.967	.318
3	.992	.895
4	.106	.387
5	.005	.848
6	.817	.889
7	.399	.356
8	.756	.989
9	.398	.662
10	.059	.094
11	.326	.853
12	.788	.497
13	.359	.313
14	.969	.833
15	.700	.861
16	.931	.632
17	.952	.967
18	.751	.969
19	.695	.632
20	.837	.344
21	.556	.833
22	.128	.889
23	.557	.862
24	.790	.960
25	.916	.248
26	.757	.030
27	.945	.951
28	.337	.899

A comparison of these figures with those obtained by Fernberger shows that, as stated above, the two criteria of approxi-

[8] Dr. Fernberger kindly provided me with his figures for squares of the deviations and the theoretical percentages (involving over half of the calculation necessary for the results given here). For the remaining computations I am indebted to my wife, Enid Rosabelle Rich, and to Miss Vergie M. Pickens.

mation sometimes yield divergent results. It will be remembered that one criterion (Urban's) is a measure of poorness, the other (Thomson's) of closeness of agreement, so that we should expect that, for any pair of groups considered, the sum of the squares should be the larger for one group and P the larger for the other group. In the majority of cases, this relation holds. But a few exceptions will test the rule and prove it to be untrue. For example, in Table I, Lighter Judgments, the sum of the squares is larger for Group 37 than for Group 36. But P is also the larger for Group 37. Thus, by one criterion Group 37 is a poorer approximation to theory than Group 36, while by the other criterion it is a better approximation. Other examples (chosen at random) are Groups 10 and 11, Table 1, Heavier; Groups 4 and 5, Table 2, Lighter; Groups 14 and 15, Table 2, Heavier. It is needless further to multiply examples. The lack of agreement between the two criteria is self-evident.[9]

We may next consider the effect of practice as shown in these Tables. The variation from group to group is entirely too great to admit of any conclusion being drawn by inspection of the figures. Boring[10] attempted to find whether or not any general tendency existed in the midst of the variability of Fernberger's results. He fitted straight lines by the method of least squares to the values given by each set of experiments, and found that in two cases the slope of the line was negative (increased approximation with practice) and in the other two cases positive (decreased approximation). In the same way, we have adjusted straight lines to our four sets of figures by the method of least squares. But in this case a positive instead of a negative slope will indicate that the agreement becomes better as practice increases.

Table	Class of Judgment	Equation of Adjusted Line
1	Lighter	$y = .0028x + .6050$
1	Heavier	$y = -.0063x + .6638$
2	Lighter	$y = .0071x + .5304$
2	Heavier	$y = .0020x + .6429$

[9] *Cf.* J. L. Ernst, F. E. Smith, L. R. Moessner, E. S. Rudisill and M. J. Atwater, this JOURNAL, 35, 1924, 255-261. These investigators tested the fit of a number of curves to mnemometric data by both methods, and their results show a number of cases in which the two criteria lead to opposite conclusions.

The probabilities (P) found above are much higher than those quoted by Thomson (Brown and Thomson, *op. cit.*, Chap. IV), which are also for lifted weights. But Thomson uses data for 450 observations at every comparison stimulus, while our groups are of 50 observations each, and the value of P depends upon the size of the sample (number of observations).

[10] E. G. Boring, Is there a Generalized Psychometric Function? this JOURNAL, 35, 1924, 75-78.

In the equation of these lines, x is the successive group (of 50 series of observations) as given in the 1st, and y the value of P as given in the 2nd and 3rd columns of Tables 1 and 2.

We are concerned with the slope of the line, which is given by the coefficient of x. It will be seen at once that this coefficient is positive in three cases and negative in one case. What significance may be attached to this fact? Probably none at all. While it is true that in 75% of the cases studied the closeness of approximation of the data to the *phi-gamma* hypothesis increases with practice, yet it must be remembered that only four cases have been studied. And four cases are surely not enough to justify the use of probabilities. We cannot even say that the chances are 3:1 that approximation increases with practice. The sample taken for study is too small to admit of generalization.

It is to be noted that we find three positive slopes out of the four, while Boring found two negative slopes. Here, again, the divergence between the criteria used is evident. In one case they show opposite results of practice from the same experimental data. The slope of the line adjusted to the values for the heavier judgments of Table 2 is positive for both Boring and us, whereas it should be positive in one case and negative in the other. The result obtained is a function of the mathematical procedure (ultimately of the basic assumptions upon which the procedure is built). One method of calculation shows increased approximation with practice, the other method decreased approximation.

Boring[11] suggests that where the scatter is so great it is unfair to represent the practice curve by a straight line. Some form of combination of adjacent groups seems to be called for. We have attempted a rough sort of combination by averaging the values of P for the two halves of the experiment (of 14 groups each). For comparison, similar averages of the sums of the squares are also given.

Table	Judgment	P		Sum of Squares	
		1st 14	2nd 14	1st 14	2nd 14
1	Lighter	.592	.700	.0143	.0117
1	Heavier	.544	.599	.0122	.0109
2	Lighter	.520	.704	.0136	.0091
2	Heavier	.628	.720	.0112	.0092

It will be noted that in every case P is larger and the sum of the squares smaller for the second 14 fractions than for the first 14. But we must be most cautious in attaching meaning to this fact, if, indeed, any meaning may be attached to it. The

[11] E. G. Boring, *op. cit.*, 76.

mathematical procedure must be kept in mind. Although each value given is for 700 judgments (14 × 50) at every comparison stimulus, it does not represent the closeness of fit of the psychometric function for the entire 700 series, but the average closeness of fit of 14 separate psychometric functions each for 50-series, a very different matter. Once more we see the effect of method of treatment upon the result obtained. The general trend of the values of P for the heavier judgments of Table 1 is to decrease, yet the second 14 values have a higher average than the first 14. A similar situation holds in two of the four cases when the sums of the squares are considered. Which method of smoothing out the irregularities (least squares or averaging) is the more legitimate scientifically? We do not know.[12]

No general conclusion as to the effect of practice seems possible from the data at hand. Some method of treating the results so that the extreme variability will not mask any possible effect of practice is obviously necessary. One method tried, least squares, leads to equivocal results and is objectionable because it attempts to fit a straight line to a progression that is probably non-linear. The other method used, averaging, yields consistent findings but its mathematical soundness may be doubted for a number of reasons. A more direct method of combination, not open to any of these objections, would be to regroup the original experimental results (percentages of judgments in each category) into groups of, say, 500- or 1000-series instead of 50-series, and then to calculate the limens, measures of precision and P's for these larger fractions. The results should be instructive since the variability of the finer fractionation that has been used ought to be minimized, and the number of limens involved is not so great as to make the work unduly laborious.

The point that we wish to emphasise is that there are two measures of approximation which at times yield divergent results. They are based upon somewhat different mathematical assumptions. The more nearly the basic assumptions of a method square with the facts which are to be treated mathematically, the more adequate will the resulting procedure be to the problems that are to be solved. In this respect, Thomson seems to have shown the superiority of his adaptation of Pearson's test for goodness of fit as a measure of the approximation of experimental data and hypothesis.

---

[12]We have also computed the averages of P for quarters (7 groups) and sevenths (4 groups) of the total. In these cases the variability has not been smoothed out and the progression is uneven. These averages, therefore, are not of sufficient importance to be given here.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. TITCHENER

LXXIII. THE DETERMINATION OF MEMORY SPAN BY THE METHOD OF CONSTANT STIMULI

By J. P. GUILFORD and KARL M. DALLENBACH

The many investigators of the problem of immediate memory have not only worked with a variety of materials and procedures, but have also complicated matters by their use of different methods[1] of evaluating their data. So greatly do these methods differ that there is little in common between the values reported.

A review shows that the methods used in the evaluation of the data fall into three groups; standards of comparison are determined by 'raw' scores, that is, by the number of items correctly reproduced; by 'weighted' scores, credit being given for partial successes; and by 'calculated' scores, derived by treating the data by certain accepted formulae. Every one of these groups shows a number of variants.

Raw Scores. There are 16 different methods in which the reports have been evaluated from the raw scores.

(1) The earliest method, and that most frequently followed, is that of Jacobs,[2] who in 1887 began the investigation of immediate memory. Jacobs presented series of materials of various lengths, gave 2 series of every length, and accepted the longest series correctly reproduced in either trial as the limit or "span of prehension." No account was taken of the series in which transpositions, misplacements, or partial reproductions were given. This method was used also by Galton;[3] by Binet and Simon in their 1905[4] scale for evaluating the reports for 3 digits, and in their 1908[5] and 1911[6] scales for determining immediate memory for digits of 4 year old children; by Yerkes and Bridges,[7] and by Humpstone,[8] Mitchell,[9] Ide,[10] Leaming,[11] Starr,[12] Brotemarkle,[13] and Clark.[14]

(2) The first variation from Jacobs' method was introduced by Münsterberg[15] in 1890. He had series of 4-10 letters read to him, 10

---

[1]A. Pohlmann, *Experimentelle Beiträge zur Lehre vom Gedächtnis*, 1906, 17-31; 42-56.
G. M. Whipple, *Manual of Mental and Physical Tests*, 2, 1915, 150-204.
S. H. Watkins, Immediate Memory and its Evaluation, *Brit. J. Psych.*, 7, 1915, 322-329.

[2]J. Jacobs, Experiments on 'Prehension', *Mind*, 12, 1887, 75-79.

[3]F. Galton, Supplementary Notes on "Prehension" in Idiots, *Mind*, 12, 1887, 79-82.

[4]A. Binet and T. Simon, Methodes nouvelles pour le diagnostic du niveau intellectuel des anomaux, *L'Année psychologique*, 11, 1905, 191-244.

[5]Le developpement de l'intelligence chez les enfants, *ibid.*, 14, 1908, 1-94.

[6]Mesure du niveau intellectuel, *ibid.*, 17, 1911, 145-201.

[7]R. M. Yerkes and J. M. Bridges, *A Point Scale for Measuring Mental Ability*, 1915, 142-143.

[8]H. J. Humpstone, *Some Aspects of the Memory Span; a Study in Associability*, 1917, 10.

[9]D. Mitchell, Variability in Memory Span, *Jour. Educ. Psych.*, 10, 1919, 448.

[10]G. G. Ide, The Educability Level of Five-Year-old Children, *Psych. Clinic*, 13, 1920, 146-172.

[11]R. E. Leaming, Tests and Norms for Vocational Guidance at the Fifteen-Year-Old Performance Level, *ibid.*, 14, 1922, 200, 207.

[12]A. S. Starr, An Analytical Study of the Intelligence of a Group of Adolescent Delinquent Girls, *ibid.*, 14, 1922, 148.

[13]R. A. Brotemarkle, Some Memory Span Test Problems: An Analytical Study at the College-Adult Level, *ibid.*, 15, 1924, 229-258.

[14]A. S. Clark, Correlation of the Auditory Digit Memory Span with General Intelligence, *ibid.*, 15, 1924, 259-260.

series of every length, beginning with the smallest and progressing to the largest, and measured his span by the longest series that he correctly reproduced in any one of the 10 trials.

(3) Bolton[16] in 1892 introduced the percentile measure. 12 trials at every series-length (digits of 5-8 places) were given the *S*s, and from the proportion of correct trials the probability of error for every trial for every series-length was computed and was used as a standard of age, school-grade, intelligence, etc.

(4) Percentile measures were also used by Bourdon[17] in 1894. The percentage of correct trials for every series-length was calculated for the different experimental groups and was used for comparative purposes.

(5) Hawkins[18] in 1897 used the percentile measure in a third manner. Series of constant lengths were presented to the *S*s, and the percentages of items correctly reproduced, apparently without regard to the serial order, were used for comparison. This method was also used by Pohlmann.[19]

(6) Binet and Henri[20] in 1895 took as their standard of comparison the number of repetitions of a series of 12 digits necessary for an errorless reproduction.

(7) Johnson,[21] also in 1895, accepted as the standard "the limit of the power of the memory to reproduce from a single hearing or seeing, immediately and without error, a succession of figures, nonsense syllables, or letters, etc." Johnson's method of determining the span is very similar to Jacobs'; it differs in that only one trial, instead of two, was given at every series-length. Gates[22] used the same method as Johnson, but, as he found that a *S* often failed at one length of series and succeeded at a longer length, he modified the method by adopting the rule "When there is but one failure followed by a success the latter is taken as the span."

(8) An eighth method was employed by Netschajeff[23] in 1900. Netschajeff presented to his *S*s series of 12 impressions (objects, noises, numbers and words denoting various kinds of ideas—visual, verbal, tactual, emotional and abstract) and took, as the measure of his reports, the number of items correctly reproduced. The method was also used by Lobsien,[24] who reduced the number of serial impressions to 9; and by Reed,[25] who used it as one of his alternative measures.

(9) A method in which the correct reports and errors were systematically classified was introduced by Smith[26] in 1903. 3 series of letters of various lengths were read to the *S*s, and the reports from immediate memory were classified for every length of series for every experimental group of *S*s in 10 different ways. Averages were figured for the number

---

[16]H. Münsterberg, Die Association successiver Vorstellungen, *Zeits. f. Psych. u. Phys. d. Sinnesorg.*, 1, 1890, 99-107.

[17]T. L. Bolton, The Growth of Memory in School Children, this JOURNAL, 4, 1892, 363.

[17]B. Bourdon, Influence de l'âge sur la mémoire immédiate, *Rev. philos.*, 38, 1894, 148-167.

[18]C. J. Hawkins, Experiments on Memory Types, *Psych. Review*, 4, 1897, 290.

[19]*Op. cit.*

[20]A. Binet and V. Henri, La psychologie individuelle, *L'Année psychologique*, 2, 1895, 442.

[21]G. E. Johnson, Contribution to the Psychology and Pedagogy of Feeble-Minded Children, *Ped. Sem.*, 3, 1895, 268, 269.

[22]A. I. Gates, Mnemonic Span for Visual and Auditory Digits, *Jour. Exp. Psych.*, 1, 1916, 393, 394.

[23]A. Netschajeff, Experimentelle Untersuchungen über Gedächtnisentwickelung bei Schulkindern, *Zeits. f. Psych.*, 24, 1900, 321-351.

[24]M. Lobsien, Experimentelle Untersuchung über die Gedächtnisentwickelung bei Kindern, *ibid.*, 27, 1902, 40.

[25]H. B. Reed, A Repetition of Ebert and Meumann's Practice Experiments on Memory, *Jour. Exp. Psych.*, 2, 1917, 320.

[26]W. G. Smith, The Range of Immediate Association and Memory, *Arch. of Neurol.*, 2, 1903, 767-805; also A Comparison of Some Mental and Physical Tests in Their Application to Epileptic and to Normal Subjects, *Brit. J. Psych.*, 1, 1905, 243f, 248.

of letters (1) correctly reproduced and rightly placed; (2*a*) correctly reproduced, but transposed with group; (2*b*) correctly reproduced, inversion, right position; (2*c*) correctly reproduced, inversion, wrong position; (3) correctly reproduced, wrongly placed; (4) omitted; (5) inserted; (6) repeated; (7) short number of letters given; and (8) excess in number given.

(10) Binet and Simon, as has already been noted, used the method of Jacobs in establishing the memory span at the lower ranges. In the higher reaches, however, in all of their scales, a different method was used. 3 series of every length were given the *S*s, and 1 correct reproduction out of the 3 trials marked a success. This is the method of most mental tests. It is continued by Terman[27] and Kuhlmann[28] in their revisions of the Binet-Simon scale.

(11) As in the experiments of Binet and Simon, Hentschel,[29] in 1912, presented 2 series of each of the shorter lengths (2 and 3 consonants), and 3 series of every one of the longer lengths (4-7 consonants). He took as his standard, however, the greatest number of consonants reproduced in any one of the 3 trials of the various series. Thus, if a *S* reproduced one of the series of 3 consonants correctly, and 3 consonants in the 4-member series, and 4 in the 5-member series, Hentschel regarded the span of that particular *S* as 4.

(12), (13) Watkins[30] in 1915 used 2 new methods: one with numerals and the second with nonsense syllables. He presented both kinds of materials visually in rows of from 2-10 units, 5 rows of every length for the numerals, and 10 rows of every length for the syllables. "The threshold is determined by the longest complete row of units which can be immediately and correctly repeated." This statement, however, is not clear; for it is apparent from Table I, in which he gives the thresholds and the number of correct reproductions for every series-length for every *S* (p. 328), that he does not mean that the threshold is determined by the 'longest row correctly repeated' in one of the 5 or 10 trials, because in no instance is the threshold placed at this value. His determination of the threshold appears to be based, for numerals, upon that value which is correctly repeated in 4 of the 5 trials; and for syllables, upon that value which is correctly repeated in 6 of the 10 trials.

(14) Mitchell[31] in 1919 used likewise 2 methods of grading his reports. He presented 2 series of 3-10 place numbers and scored the reproductions by Jacobs' method, as we have already seen, and by a second and new method in which he determined the score by "the longest series, in both attempts at which the child was successful at reproduction and in no series previous to which he had failed."

(15) The next method was introduced in 1923 by Starr,[32] who presented 4 series of digits of every length, beginning with the 3-place numbers and progressing with the success of the *S* to the higher. "When the response was correct another series increased by one digit followed, and this procedure was repeated until the child failed to recall correctly. Two correct responses in four trials were required, in order to eliminate the possibility of chance."

(16) The most recent method is that of Easby-Grave,[33] 1924. Using series of digits of varying lengths, she determined the memory span by the position of the first true failure. "If the child fails on a series of digits,

---

[27]L. M. Terman, *The Measure of Intelligence*, 1916.
[28]F. Kuhlmann, *A Handbook of Mental Tests*, 1922.
[29]M. Hentschel, Die Gedächtnisspanne, *Zeits. f. pād. Psych.*, 13, 1912, 562-579.
[30]S. H. Watkins, Immediate Memory and its Evaluation, *Brit. J. Psych.*, 7, 1915, 319-349.
[31]D. Mitchell, Variability in Memory Span, *Jour. Educ. Psych.*, 10, 1919, 445-457.
[32]A. S. Starr, The Diagnostic Value of the Audito-Vocal Digit Memory Span, *Psych. Clinic*, 15, 1923, 61-85.

he is given another series of the same length. If he also fails the second, he is given a trial at the next longer series. If he fails this we consider that the first failure showed the true limit of his span."

A summary and classification of the methods of evaluating the 'raw' scores reveals the following types and sub-types. Immediate memory is measured:

I. By the length of the longest series correctly reproduced
   (*a*) in 1 trial for every series-length (7th method);
   (*b*) in 2 trials " " " " (1st ");
   (*c*) in 3 " " " " " (10th ");
   (*d*) in 10 " " " " " (2nd ");
   (*e*) 2 times in 2 trials for every series-length (14th method);
   (*f*) 2 " " 4 " " " " " (15th ");
   (*g*) 4 " " 5 " " " " " (12th ");
   (*h*) 6 " " 10 " " " " " (13th ").

II. By the probability of error at every series-length (3rd method).

III. By the percent. of correct reproductions
   (*a*) of series (4th method);
   (*b*) of items (5th method).

IV. By the number of items correctly reproduced
   (*a*) average (8th method);
   (*b*) greatest number in one of 3 trials (11th method).

V. By the number of repetitions necessary for an errorless reproduction (6th method).

VI. By a qualitative analysis and classification of the correct reports and the errors (9th method).

VII. By the first true failure (16th method).

Weighted Scores. The methods of evaluating the reports from the 'raw' scores did not take into account the manner in which the reproductions were given. It was for the correction of this defect that graded systems of marking were adopted. 9 different methods of weighting the *Ss*' reports have been employed.

(1) (2) Smith[34] in 1895 introduced the method of weighting. He used two systems which complemented each other; "each took into account what the other neglected." The first method consisted merely in counting the errors of (*a*) omission, (*b*) addition, and (*c*) transposition. "Each error has the same weight attached to it and counts 1; the total number of errors gives a basis for estimating the work of memory in any instance." The second method consists in an arbitrary assignment of marks; 1 mark was credited for every unit "given in an entirely wrong position, or its position is quite unknown;" 2 marks for every unit whose position was only imperfectly known, *e. g.*, "the letter is put in the right line, or in its correct place in a group of letters which is in a wrong position"; and 3 marks were credited when "everything is right." Comparisons of the scores thus obtained were made among the *Ss*. This later method was used by Winch[35] in both of his studies of immediate memory.

(3) Ebbinghaus[36] in 1897 likewise computed his results by 2 different methods. The first, a rough and ready method, consisted merely in counting the incorrect series, without regard to the number or kind of error.

---

[34]C. Easby-Grave, Tests and Norms at the Six Year Old Performance Level, *ibid.*, 15, 1924, 268-269.

[34]W. G. Smith, Relation of Attention to Memory, *Mind*, 4, 1895, 51, 52.

[35]W. H. Winch, Immediate Memory in School Children, *Brit. J. Psych.*, 1, 1904, 127-134; also Immediate Memory in School Children, *ibid.*, 2, 1906, 52-57.

[36]H. Ebbinghaus, Über eine neue Methode zur Prüfung geistiger Fähigkeiten, *Zeits. f. Psych.*, 13, 1897, 423.

The second, the third weighted method to be proposed, took account of these factors. Ebbinghaus counted every incorrect item and omission as 1 error and every transposition as 1/2 error, added together these values, and took the sum as a comparative measure of his *Ss* immediate memory. Ritter[37] subsequently used the same procedure.

(4) Ebert and Meumann[38] in 1904 introduced a fourth method. They counted omissions and additions as 4/4 error, displacements in the series of more than one place as 3/4 error, displacement of one position as 2/4 error, and a correction as 1/4 error. This system of marks was later used, for comparative purposes, by Reed.[39]

(5) Meumann[40] in 1905 used another method. He counted an omission and a totally incorrect substitution as 3/3 error; a transposition of 2 places and a substitution which was acoustically similar to the given item as 2/3 error; and a transposition of 1 place, and for the youngest children every transposition, as 1/3 error.

(6) Burt[41] in 1909 used a method of marking based, he says, on Meumann's system; in fact it more nearly resembles the converse of Ebert and Meumann's method. "Each word correctly reproduced in its correct place counted 4, a correct word misplaced counted 3 if its position was altered by only one place, and 2 if removed by more than one degree; an incorrect word, if either initial consonant sound, or final consonant sound, or medial vowel sound alone was incorrectly altered, counted 3, if in its right place; 2, if one place removed, and so on; if two such components were altered, it counted 2 if rightly placed, and 1 if wrongly placed; the omission of a word, or the substitution of an extraneous word, counted 0."

(7) Pyle,[42] in 1913, adopted the simple method of giving one credit for every item correctly reproduced and a second credit for every item placed in its correct position.

(8) Dallenbach[43] in 1914 gave one credit for every item correctly reproduced and deducted from the total 1/2 credit for every error of transposition or insertion.

(9) Because the rules thus far devised made no allowance for crediting a correct sequence of items in the wrong part of a series, Woolley and Fischer[44] in 1914 introduced a new set of rules and system of weights: a correct digit in the correct place counts 2; in series in which there is but one mistake, whether of omission, addition or misplacement, 2 is deducted from the possible score; in series in which more than one error occurs (*a*) 2 is counted for every correct digit in the correct place, and 1 for every correct digit on place removed; (*b*) no credit is given for a correct digit more than one place removed unless it forms part of a correct sequence of 3 or more digits, in which case a credit of 2 is given for all except the first digit, which is given no credit if it is more than 1 place removed, and a credit of 1 if it is but one place removed.

The methods of weighting may be classified into three types:

I. the negative methods, in which only the errors are weighted (1st, 3rd, 4th and 5th methods);

---

[37] C. Ritter, Ermüdungsmessungen, *ibid.*, 24, 1900, 406.

[38] E. Ebert and E. Meumann, Über einige Grundfragen der Psychologie der Übungsphänomene im Bereiche des Gedächtnisses, *Arch. f. d. ges. Psych.*, 4, 1904, 11.

[39] H. B. Reed, A Repetition of Ebert and Meumann's Practice Experiment on Memory, *Jour. Exp. Psych.*, 2, 1917, 315-346.

[40] E. Meumann, Intelligenzprüfungen an Kindern der Volksschule, *Zeits. f. exp. Päd.*, 1, 1905, 63 f, 67 f.

[41] C. Burt, Experimental Tests, *Brit. J. Psych.*, 3, 1909, 142.

[42] W. H. Pyle, *The Examination of School Children*, 1913.

[43] K. M. Dallenbach, The Effect of Practice upon School Children, *Jour Educ. Psych.*, 5, 1914, 334; also The Effect of Practice upon Feeble-Minded, *ibid.*, 10, 1919, 61-82.

[44] H. T. Woolley and C. R. Fischer, Mental and Physical Measurements of Working Children, *Psych. Mon.*, 18, 1914, 124.

II. the positive methods, in which only the correct reports are weighted (2nd, 6th and 7th methods); and
III. the mixed methods, in which the correct reports are weighted positively and the errors negatively (the 8th and 9th methods).

Calculated Scores. The methods of weighting are at best but arbitrary attempts to take account of errors and partial successes. The assignment of credits is based upon the assumption that every member of a series has an equal reproductive place-value; and this assumption is not true. The first and last members of a series, as experiments[45] have repeatedly shown, are best retained; those in the middle lie at positions of disadvantage. It is therefore an error to weight equally all the items of a report. This fact has been recognized, and two attempts have been made to avoid it. The first was introduced by Krueger and Spearman[46] in 1907 and the second by Wagner[47] in 1913. In both of these studies, correlations, based on the place-values, were calculated between the order of the impressions as reproduced and as presented. Krueger and Spearman used Spearman's 'footrule' formula, and Wagner a formula derived especially for the problem. Both methods demand separate consideration for every report of every *S*. The amount of labor involved, as Mitchell[48] has pointed out, is prohibitive where many reports have to be considered.

27 methods, of all degrees of accuracy and complexity, used in determining the efficiency of immediate memory,—and no accepted principle underlying them! It was with the hope of supplying such a principle, and of showing how the methods of psychophysics may be used in solving the problem, that the present Study was undertaken. We propose to show, as Fernberger[49] has already done for the problem of range of attention, that the principle of the statistical limen is a more reliable and more easily determined measure than any thus far proposed.

The limen of immediate memory or the memory span is defined as that length of series which has the probability 0.5 of being retained; in other words, that length of series which is as likely to be remembered as not. The method proposed for determining this statistical value is the method of constant stimuli.[50] The data of many of the Studies mentioned in the above review are susceptible to this treatment, and would yield memory-spans as thus defined. We, however, for purposes of illustrating the principle and method, have conducted two tests: an intensive study upon a few *S*s; and an extensive study upon a large class.

In both Studies series of digits of various lengths, constructed according to accepted methods,[51] were read to the *S*s, without rhythm and at a constant rate, as timed by a soundless metronome, of one digit in 0.7 sec.

In the intensive study, series of 6-12 place numbers were presented, 100 series of every length, in haphazard order to 5 *S*s, who, upon a signal given immediately after the reading of the last digit, wrote what they retained upon specially prepared report sheets. These sheets were so constructed that the *S*s could not see, in writing one report, what they had

[45] T. L. Bolton, *op. cit.*, 379; A. Binet, La mémoire des nots, *L'Année psych.*, 2, 1894, 12.
[46] F. Krueger and C. Spearman, Die Korrelation zwischen verschiendenen geistigen Leistungsfähigkeiten, *Zeits. f. Psych.*, 44, 1907, 71; cf. also G. M. Whipple, *op. cit.*, 161 f., for a description of the method based on an explanation by Spearman.
[47] P. Wagner, Über die rechnerische Behandlung der Ergebnisse bei der Prüfung des unmittelbaren Behaltens, *Zeits. f. pād. Psych.*, 14, 1913, 60-63; *cf.* also S. H. Watkins, *Brit. Jour. Psych.*, 7, 1914, 324ff.
[48] D. Mitchell, *Jour. Educ. Psych.*, 10, 1919, 448.
[49] S. W. Fernberger, A Preliminary Study of the Range of Visual Apprehension, this JOURNAL, 32, 1921, 121-133.
[50] For a description of this method *cf.* F. M. Urban, Hilfstabellen für die Konstansmethode, *Arch. f. d. ges. Psych.*, 24, 1912, 236-243; also E. G. Boring, Urban's Tables and the Method of Constant Stimuli, this JOURNAL, 28, 1917, 280-293.
[51] The 0 was not used; runs of 3 or more digits in their natural or reverse order were not given; in series of 9 digits or less, no digit was repeated; in series longer than 9, the same digit was not used in succession or at the beginning and ending.

written in earlier reports[52]. The distracting influence of other series of numbers in the field of sight was thus avoided.

The experiments extended over a period of 7 days. At the beginning of every experimental hour the following instructions were read to the *Ss*: "I am going to read a series of numbers containing from 6 to 12 digits. Listen carefully, and after I have completed the series, write in the open rectangle of your record sheet as many of the numbers in their correct order as you can remember. Some series may be so easy that you can remember all of them; others may be so difficult that you can not. Do not let a failure worry you. After writing what you remember, pull the strip of paper upward until the numbers you have written are just out of sight, and be ready to listen for the next series."

The reports were marked 'right' or 'wrong' according as the order and digits were correctly reproduced. The memory-spans and the degrees of precision were computed from the 'right' cases for every *S*. These data are given in Table I, which shows besides these values the number of trials correctly reproduced by every *S* at every series-length.

TABLE I

Showing for Every *S* the Number of Trials, at Every Series-Length, Correctly Reported, the Memory-Span, Expressed as a Limen, and the Degree of Precision

S	Series Length in Number of Digits						Limen	Precision
	6	7	8	9	10	11	12	(L)	(h)
B	67	42	26	5	2	0	1	6.713	.426
C	96	93	78	45	26	12	2	9.357	.464
E	98	97	83	77	42	29	16	9.985	.382
G	81	70	43	36	24	23	14	8.215	.226
M	93	81	68	34	21	8	5	8.592	.392

In the extensive study, a series of 4-13 place numbers was read in ascending order to 100 *Ss*. The *Ss* wrote their reproductions upon narrow strips of paper. They began at the top and, after every reproduction, folded the paper so as to turn the numbers they had just written out of sight. The reports were graded 'right' or 'wrong', and the memory-span and degree of precision were computed from the 'right' cases for the group. These data appear in Table II, which also shows the number of reports given correctly at every series-length.

TABLE II

Showing for 100 *Ss* the Number of Reports Correctly Given at Every Length of Series

Number of Correct Reports at										Limen	Precision
4	5	6	7	8	9	10	11	12	13	(L)	(h)
100	96	86	62	38	18	5	2	1	0	7.583	.446

The results of both Studies show that we are dealing with a continuous function. The frequency of correct reports is high for the short series and low for the long series. No inversions of the first order occur, and the curves of the 5 *Ss* and the curve of the group show the ogive form.

The memory spans of the 5 *Ss* vary from 6.7 to approximately 10 (9.985); the memory-span of the group is 7.583—a value almost identical with that of Gates[53] (7.666), from whose experiment the digit series 4-12, used in the extensive study, were drawn.

The advantages of evaluating the data of immediate memory by the method of constant stimuli are many. (1) The measure, the memory-span,

---

[52]Two piece of cardboard were fastened together. Between them ran a strip of paper from an odding-machine roll. In the upper cardboard a window 2 x 6 cm. was cut, and through this opening the *S* wrote upon the strip. After the report had been given, the strip was pulled upward until the window was again blank.

[53]*Op cit.*, 394.

is unequivocally defined, and the definition rests upon the accepted psychophysical principle of the limen. (2) An exact determination is obtained more readily and more quickly than by any of the methods thus far proposed; if groups are investigated, the memory span may be obtained from a single presentation of the experimental series; if studies of individuals are undertaken, 5 or 10 trials at every series length may suffice.[54] (3) All the reproductions, failures as well as successes, are taken into account. A success at a long series is not disregarded because of failures at shorter series. (4) Complicated rules and weights are unnecessary. (5) Single representative values with their degree of precision are obtained. (6) Comparisons may be made between individuals or groups from two points of view: liminal values and the amounts of dispersion may be compared.

[54] E. G. Boring, The Number of Observations upon which a Limen may be Based, this JOURNAL, 27, 1916, 315-319.

BOOK REVIEWS

The Four Daughters of God: a Mirror of Changing Doctrine. By Hope Traver. Modern Language Association of America, 1925. pp. 92.

The Alexandrian method of Biblical interpretation, which uses a clause in literature as a metaphor, as other writers use objects in nature, would open an interesting investigation to the psychologist. We know today that this method, which has never died out in the church, has its inner source in two unlovely traits of personality,—in indolence, which substitutes the fascination of ingenious mind-play for the patient labor of historical research; and in vanity, which flatters itself that God has revealed to certain favorites inner truths which the rest of the world is too spiritually torpid to discover.

It is this type of mind that has fastened on such passages of the Bible as that which it names "The Four Daughters of God". In the work before us, Dr. Traver, in a brilliant survey, has used this famous allegory as a mirror, in which the most important change in theological thought is traced. Dr. Traver, professor at Mills College, is a scholar of high attainments. A previous monograph, *The Four Daughters of God: a Study of the Versions of this Allegory with Especial Reference to those in Latin, French and English*, one of the Bryn Mawr College Monographs, is here carried farther, with the aid of the Dutch versions, and allied originals, in the British Museum, the Paris International Library, and elsewhere, as throwing light from a new point of view on the present controversy between the extremes of religious thought. This service is the more welcome because it treats of the central theme in Christian theology, the principle of Reconciliation.

The varied versions of the story itself, in their original languages, their Biblical sources and analogies, the patristic apocalypses, related and similar myths, as well as the influence the allegory has exerted on the theologians of both the Eastern and the Western Churches, are examined with thoroughness. The relation of the Apocalypses, particularly, to the Processus Belial, is interestingly established, and the yet more important story of the rise of the cult of the Virgin Mary, as she gradually displaces the archangel Michael as a defender of humanity before the divine seat. "Mariolatry is a natural development from three causes: (1) a tendency to conceive and worship the ideal woman; (2) the desire for symmetry, for a new Eve to counterbalance Christ as the new Adam; (3) the longing for an intercessor whose interest would be for mercy without the necessity for thought of justice."

The author's central purpose is to show the shift of emphasis in theological thinking from the juridicial to the human view of the Atonement. The reader follows, with the aid of sources hitherto too little known, the gradual change of view from the avenging justice of God to his mercy, against which both the Devil and Truth contend in a persistent but losing fight. "As opposed to these somewhat external and almost mechanical conceptions is the idea of mercy embodied in the Incarnation of Christ not so much as substitute for man in satisfaction of the law of justice or in fulfillment of the sentence of death which God's truth demanded, but as a revelation of God's self to man, and as an example of obedience and love, thereby inciting man himself to repentance and confession and to a life devoted to contemplation of God and mystic union with Him. Such are the doctrines whose changes are mirrored in the allegory of *The Four Daughters of God.*"

We shall look forward with interest to Dr. Traver's coming studies in the evolution of the allegories related to that of "The Four Daughters".

Berkeley Baptist Divinity School A. S. Phelps

L'Odorat. By H. Zwaardemaker. Paris, G. Doin, 1925. pp. 315.

This little book, published in the *Bibliothèque de psychologie expérimentale,* may be characterized as a translation of an abridged and revised edition of the author's *Physiologie des Geruchs.* It lacks the Introduction, the chapters on the Technique of Smell-measurement and on Deviations from Normal Olfactory Sensitivity, and the three sections of the Appendix. But it contains in order all of the remaining chapter-headings, and a translation of as much of the material under them as the author could use. This retention of the plan of the old book would seem to mean that in the author's mind the problems of the olfactory sense remain the same; but the thirty years that separate the two volumes have seen much new work within and without his laboratory on these problems. The technique of experimentation has been further refined, new apparatus has been devised and the old improved, the range of stimuli has been enormously increased, and the reasons for the variability of result are better understood. The outcome of all this work is written into the new parts ot the book; some of it overtly, with new figures and tables, and some of it casually in the run of discussion. The author's general conclusions, however, on all of the fundamental problems remain substantially the same; the reciprocal effects of smell-exhaustion and the compensation of odors are still 'facts', and the classification of odors is unchanged. The new note is one of greater caution as regards the significance of the quantitative results, and of emphasis on the difficulties of experimentation.

Throughout the book Zwaardermaker is, of course, always a physiologist, and he gives scant attention to writers on the psychology of smell. Henning is, with minor exceptions, mentioned only in the discussion of the classification of odors. The smell-prism is rejected for two reasons. First, the division of the *Odores aromatici* into the spicy and resinous groups is, the author thinks, unjustified, because the groups contain odors which behave similarly in small-exhaustion experiments. Furthermore, he says (mistakenly, we believe), that Henning finds no place for the amber-musk, hircine and garlic odors. In general, the author's attitude toward psychology is strikingly like that of Helmholtz. For example, unable to find either in the chemistry of the stimuli or in the physiology of the sense organ an explanation of the compensation of odors, he thinks it probable that this process is of a psychological order similar, perhaps, to Heymans' mental inhibition.

Nevertheless, psychology will be grateful for the book. It does not entirely replace the earlier work; but it is worth much to have the same problems discussed after so many years, particularly when, as in this case, the exposition is characterized by the easy familiarity, the sure touch, the ready frankness, and the authoritative tone of a master in his field.

H. P. W.

Laboratory Studies in Educational Psychology. By Egbert Milton Turner and George Herbert Betts. New York, D. Appleton and Co., 1924. pp. ix, 218.

Laboratory courses in Educational Psychology are now finding a place in the curricula of normal schools and schools of education. The problem in such courses is to decide whether one considers it of more value to take up for experimental study a wide range of subjects or to limit the work to a smaller number of experiments which are carried on by the student in a more intensive manner. In teaching such courses the reviewer has adopted the plan of taking up a relatively small number of problems, giving variations of the same problems to students, and insisting upon careful bibliographical detail. The students acquire in this manner some notion of the

experimental procedure in education and, what seems almost equally important, learn to use psychological and educational literature independently.

The present volume presents fifty-two experiments with an appendix upon educational statistics. The authors have not endeavored to describe experiments which exhaust the topics considered; the experiments are intended to be suggestive, to make general "observers" of students; and the instructor is asked "to warn the students not to generalize too much" about the experiments. The bibliography is fairly complete. Some of the experiments, owing to the simplicity of their presentation, may perhaps be misleading and harmful. For example, the casual observation of instinctive tendencies (Experiment 36) might tend to belittle the scientific difficulty of differentiating between native and learned responses. This case may be taken as illustrative of the danger involved in attempting to cover a broad range of subjects; and seems to point to the more intensive training, outlined above, as being of more value even to the average student.

Vocational Self-Guidance. By DOUGLAS FRYER. Philadelphia, J. B. Lippincott Co., 1925. pp. ix, 385.

The reviewer is at a loss to know how to approach this work upon vocational self-guidance. The first part is made up of five chapters upon vocational psychology, including a discussion of methods of self-measurement and of evaluation of personality. The second part of the book, and by far the more valuable, is given over to twenty-one chapters written by successful specialists in the various business professions. Here the material is concrete, and the values are the values of the business world. Apart from these chapters, the book seems vaguely inspirational and uplifting: broad statements are made which purport to rest upon an adequate basis in fact. But do they? Is it safe and reasonable to say, for example, that an A-man (mental age 18.0 and up) may expect occupational success as a technical salesman, while a B-man (mental age 16.5 to 17.9) may expect success as a physician? In spite of the army figures, such a statement approaches absurdity.

The reviewer can but ask whether such a book ought to be written. If there be a field of vocational guidance, does it not consist in a careful study of the individual's interests, abilities and opportunities, and an attempt upon the part of the examiner to make the individual self-reliant and independent?

Cornell University SETH WAKEMAN

NOTE

We owe the portrait of the late Professor Franz Brentano, which appears in the present number, to the courtesy of Professor Mario Puglisi of Florence, the author of the sketch of Brentano's life in our number of July, 1924. The signature is taken from the book of O. Kraus, *Franz Brentano: zur Kenntniss seines Lebens und seiner Lehre*, 1919.

INDEX

By H. Helson

AUTHORS

(The names of authors of original articles are printed in Small Caps.)

Anderson, J. E. 461
Bartlett, F. C. 449
Bethel, J. P. 102
Betts, G. H. 630
Bichowsky, F. R. 588
Burtt, H. E. 553
Brentano, F. 303
Carmichael, L. 446
Carpenter, W. R. 309
Cason, H. 371
Conklin, V. 96
Cotton, H. A. 153
Cutsforth, T. D. 527
Dallenbach, K. M. 148, 154, 312, 621
Dimmick, F. L. 96, 412
Downey, J. E. 151
Dresser, H. W. 464
Ewer, B. C. 150
Farnsworth, P. R. 394
Ferree, C. E. 171, 178, 188
Fisher, S. C. 1
Flexner, A. 465
Foster, J. C. 151
Franz, S. I. 149
Fryer, D. 631
Fukuda, T. 124
Giang, F. 456
Guilford, J. P. 615
Harding, L. 454
Helmholtz, H. von 305
Helson, H. 342, 494, 632
Herring, J. P. 151
Heymans, G. 464
Houdini, H. 148
Hunter, W. S. 286
Ives, M. 456
Jacobson, E. 73
Johnson, H. M. 601
Kohs, S. C. 151
König, T. 465
Lidell. H. S. 544
Malmud, R. S. 401
Mateer, F. 146
McGeoch, J. A. 434
Meumann, E. 306
Möller, E. 249
Monroe, M. 192
Moore, T. V. 307
Mull, H. K. 469
Myers, C. F. 151
Myers, C. S. 53
Myers, G. C. 151
Neterer, I. M. 151
Nixon, H. K. 418
Ogden, R. M. 140
Osborn, H. F. 151
O'Shea, M. V. 147
Pisek, F. 459
Pollack, M. 456
Rand, G. 171, 178, 188
Rich, G. J. 466, 597, 615
Rosenow, C. 233
Rudisill, E. S. 562
Scahill, H. G. 412
Simons, H. 454
Spencer, L. T. 427
Stedman, L. M. 146
Stern, E. 307
Stern, W. 308
Stone, A. R. 113
Strong, E. K. 307
Stumberg, D. 88
Tinker, A. 467
Titchener, E. B. 156, 157, 311, 313, 468
Tomlinson, D. 454
Traver, H. 629
Travis, R. C. 207
Turner, E. M. 630
Tuttle, W. W. 553
Washburn, M. E. 454, 456, 459
Weber, C. O. 66
Wechsler, D. 424
Wiegmann, O. 308
Wilcocks, R. W. 324
Wilson, R. D. 151
Yerkes, R. M. 151
Young, P. C. 214
Zwaardemaker, H. 630

SUBJECTS

Abilities, 103, 299; general and specific, 64; mental, moral, 65; in hypnosis and waking, 231 f.; musical, 474, 491; transfer of 478
Abnormal, 6; psychology 64
Abstraction, 170, 312; Külpe's experiments on, 324 ff.; in physics 170
Acquired characters, 46; dispositions 48
Act, 240 f., 291, 345; of knowing, 53; psychical, 506; special, 349 ff.; social and non-social 242
Act-psychology 343 f.
Adaptation, 57, 165, 432 f.; of eye 191
Adjustment, 31, 166, 293; attentional, 602; degree of, 604; neural scheme of, 602 ff.; sensori-motor 601
Adolescence, 21 ff.; neo-atavistic 42, 46
Aesthetic enjoyment, 42; proportion 92, 198 ff., 206
Aesthetics, 19; of color 205 ff.
Affection 143
Affective method, demonstration of, 311 f.; quality of color, 205; reaction, 59; states, and memory, 112, 114 ff., 120 f.; time and patellar reflex 553 ff.
After-image 7
American Journal of Religious Psychology and Education 44
American Journal of Psychology 18
American Psychological Association 20
Amnesia 207 ff., 217
Analysis, 4 f., 26, 34, 96, 141 ff., 167, 245, 299, 323, 344, 360, 494 ff., 500; functional, 348, 353, 501; qualitative, of reaction words, 92 f.; rejection of 345 ff.
Anger, recall of 457
Angewandte Psychologie 307
Animal psychology 167, 342
Anthroponomy 286 ff.
Anthropology 19, 48, 50, 290 ff.
Aphasia 63
Apparatus, 161 f., 168, 170, 319; for affective demonstration, 311 f.; for apparent movement 446 ff.
Apperception 10, 32, 315, 317, 344, 351
Appetite, 43; appetition 59
Applied psychology 64, 146, 150, 156, 307, 320
Apprehension, 154 ff.; of experience, 140 ff.; of power and matter 356
Association, 20, 53, 317 f., 343, 345, 363, 373, 376, 378, 380, 382, 390 ff., 401, 455 f., 489, 497, 532; areas, 380; and attention, 357; and guilt, 93; in hypnosis, 225; necessary for absolute pitch, 471, 492 f.; restatement of, 354 ff.; reaction experment, 553; reaction method, 88 ff.; time 8, 217
Attention, 6, 14 f., 28, 30, 61, 73, 75, 162, 165 f., 170, 251, 283, 313, 318, 334, 337, 340 ff., 350 ff., 356 ff., 431 f., 407, 496, 502, 504 ff., 564, 592; and absolute pitch, 477, 480, 482 ff., 487, 491 f.; and association, 357; as adjustment, 602, 604; as clearness, 601 ff.; definition of, 601 ff.; degree of, 601 ff.; law of, 140; meanings of, 356 ff.; Müller's use of, 357; measurement of, 601 ff.; and muscular tenseness, 85, 87; periodicity of, 102 f.; range of 154 ff.
Attitude, 61, 201, 216, 230 ff., 346 f., 359; analytic, 495, 503 f., 506; attentive, 602; constancy of, 562 ff., process, 562 ff.; purity of, 568 ff.; stimulus, 415, 562 ff.; synthetic, 495, 506; in weight perception 562 ff.
Attributes, 542; clearness, 608; tonal 469 ff., 475, 477, 492
Aufgabe, 330 f., 341, 352, 476, 483 f., 492; and abstraction 324 ff.
Aussage test 434 ff., 444
Automatic writing, 54; movements 170
Automatism 23, 25, 28, 34, 45

Barometric changes and work 105, 108 f., 111
Behavior, 25, 48, 55 f., 59, 64, 233 ff., 287 ff., attentive, 606 ff.; intelligent, 349; meaning of, 236; patterns, 247;

norms of, 48; of sheep and goats in maze learning 544 ff.
Behaviorism, 234, 236, 245, 248, 290, 297, 306; and meaning 233 ff.
Binet-Simon tests 125
Binocular vision 168, 170, 523
Biometry 66
Blood-pressure, 14, 115, 118 ff.; and examinations, 116; and memory 116
Brightness, 192, 194 ff., 198 ff., 204, 206, 431

Causality, 27, 70, 418; psychic 68, 287
Cerebral paralyses 149
Chance, 58, 67 f., 71, 328; reality of, 66; theories of 349
Character, 130; and associates 132, 419
Characters, acquired 46
Chess, psychology of 166
Child, care of, 147; in home, 130, 133; nature of, 147; overstudious, 24; paleo-atavistic, 46; study of 18 ff.
Childhood, 32, 41, 45; experience of 528
Children, 170; language of, 168; normal and abnormal, 434 ff.; superior 146
Christianity and evolution 151
Chroma, 192, 194 ff., 198 ff., 204, 206
Chronoscope, 162; vernier 164, 168
Clang patterns 476
Clearness, 154, 347, 496, 519, 601 ff.; attributive, 358 f., cognitive, 155; definition of, 144; degree of, 605, 612; sensory, 155; variation 144
Clinical psychology 147
Cognition, 54, 140 ff., 154 f.; determinateness of, 144; and emotion, 541; and feeling 527 ff.
Color, affective quality of, 205; aesthetics of, 205 ff.; emotional, 538 ff.; form, 535; induction, 496; preference, 205; surface and film, 496 f.; theory of G. S. Hall, 7; weight of, 192 ff., 202, 205; vision 55
Comic, experiments on, 209 ff.; and falsehood 309
Conative control 144
Concepts, descriptive and functional 519
Conditioned reflex 343
Conditioned response, physical basis of 371, 376
Configuration 8, 143 ff., 342 ff., 495 ff.
Conjuring 167
Consciousness, 11, 16 f., 24, 29, ff., 48 f., 45 ff., 62, 73, 75, 141, 155, 167, 213, 215, 233, 238, 243, 286 ff., 290, 292, 294, 297, 318, 516, 534 f.; affective modes of, 60; centre of, 62; comparison, 585; criteria of, 32; degree of, 601; function of, 60; higher, 46; hypnotic, 230, 232, 235; kinds of, 63; limitations of, 47; loss of, 63; mechanism of, 588 ff.; problems of, 298 ff.; relational, 355 ff., 368; twilight, 217, 222; of self, 55; splitting of, 62; and stimulus, 588 ff.; theory of, 74; threshold of 28
Constancy, of attitude in weight perception, 562 ff.; law of, 508; hypothesis 350
Constant stimuli, method of, applied to memory span 615 ff.
Context, 235, 539; of emotion 100
Contour, 143; definition of, 143, 145, 283 f.; of fields 497
Contrast, 195, 345, 431; lines 262
Conversion 41 ff.
Cortical configuration, 144; tension 144
Creative resultants, principle of 345
Death 44, 167
Defectives, 26; types of mental 154
Dementia praecox 154
Depression 59, 454
Depth, perception of 520 ff.
Dermal blind spots, 13; sensations, 13; sensitiveness 14
Description, 299; analytical, 97 ff.; phenomenological, 497; problem of 495
Determinism 67, 69
Development, 26, 35, 42 ff., 293; mental, 53; sexual 41
Diagnosis and I. Q. 146
Differential limen, 427 ff., 461 ff.
Digit span 223, 615 ff.
Discrimination, 54, 170, 231, 367; of pitch, 470; in hypnosis 226 ff.
Dislikes, recall of 112, 114
Disparate images 520
Disposition, 113; inheritance of 48
Dissociation, 54, 207, 208, 210, 212, 217, 231, 382; in nerve tissue 381

Distraction, 468; auditory 478, 487
Diurnal efficiency 104
Double personality 212
Dreams 7, 54, 170
Duration 235

Education, 7, 21, 24, 45, 147, 149, 150, 158, 169, 291; industrial 37
Educational psychology 64
Emotion, 54, 56, 73, 75, 96, 115, 117 f., 121, 162, 318, 424, 527 ff.; and blood-pressure, 116; colors, 531; and cognition, 541; context of, 100; core of, 100; description of, 96; intensity of, 460; and memory, 116, 118; and movement, 100; physiological changes in, 96; recall of, 456 ff.; rôle of, in a synaesthetic subject, 527 ff.; and unpleasantness 100
Emotional coloration, 542; reactions, 424 ff.; synaesthesis, 529; pattern 100
Empirical psychology 315 ff., 322
Energy, 36, 56, 57; drainage of, 62; mechanical, 58; mental 143
Environment, 57 f., 60, 64, 124, 130 f., 239 f., 243; and intelligence 124
Epicritic system 54
Equality-of-brightness, 171 ff., 175 ff., 188 ff.; photometry 178
Errors, average, 9; octave, 472 ff., 484, 486; probable 9
Evolution, 22, 27, 43, 54, 57 f., 62, 72, 234, 237; and Christianity, 151; of cutaneous sensibility 54
Experience, character of, 141; form of, 140 f.; of muscular contraction, 77, 85; of the race 30
Experimental psychology, 313 ff., 363, 370, 495
Explanation, 300 ff.; in anthroponomy, 301; cause of error, 545; genetic 35
Expression, method of 311
Eye-movement, 250 f., 283; theories of 500

Facilitation 61
Faculty, 26; mythopœic, 33; totalizing 33
Familiarity, feeling of 490 f.
Fatigue 6, 16, 38, 61, 92, 108, 111, 144, 215, 221, 230 ff.
Fear, 39, 59, 96, 243 ff., 419, 422; recall of, 457 ff.; as unpleasant, 97 ff.; pattern of 100
Feeling, 21, 30, 32, 37, 43, 54, 344, 453, 589; of activity, 317; as undifferentiated cognition, 543; of familiarity, 490 f.; feeling-instincts, 28, 31; relation to cognition process, 523 541; sexual 59
Fidelity of report 434
Film color 495 f.
Fixation, effect of, on movement, 501 ff., 506; point, 260 ff., 283
Flicker 171, 173, 175, 188, 511
Forgetting, rate of 486
Form 344 f., 348, 360, 356, 359, 361, 504
Form-quality 343 f., 360
Forms, schematized for number, 528; month, 530; skeleton, 252, 262; mirrored, 252, 273; geometric 271
Freudism 43, 150
Function 153, 214, 297
Functional variation 494
Functions, complex and simple, 56; vital, mental 57
Fusion, limen of, 508; and movement, 510 ff.; optical 507

Galvanometer tests 455 f., 459 ff.
Gegenstandstheorie 353
General emotivity, 424, 426; factor of intelligence, 143 ff.; intelligence, 62; psychology 64
Genetic law 46
Genius 39, 54, 166
Gestalt, psychology of 342 ff., 494 ff.
Ghosts 66
Glandular activity, 16; secretions 55
Gregarious instinct 544, 547, 549
Grief, 153; and memory 121

Habit, formation of, 49; in sheep and goats, 544 ff.; patterns 37
Habits, 28, 38, 42 f., 130, 149; inherited, 58; motor 149
Habitual motivations 312
Hallucinations 33
Hall, G. S., psychological and educational work of 1 ff.
Health, 38; curve of 42
Hedonic scale 533
Hedonism 72
Hemianopsia 346 f.
Hemiplegia 207
Heredity, 42, 60; mental 49
Heymans' law, validity of 427 ff., 461 ff.
Horopter 161

Humidity and work 105 f., 108 ff.
Hunger 28, 43
Hypnosis 6, 207, 214 ff.

Idealism and eyemindedness 34
Ideas, repressed 88
Ideational types 32
Illumination 25 f., 262, 277, 511 f.
Illusion, 54, 202, 205, 448, 505; of judgment, 502; Müller-Lyer, 520; staircase, 346; Zöllner 520
Image, 54, 77, 80 ff., 89, 98, 100, 167, 247; level, 592; process, 87; and relaxation, 80 ff.; stereotyped forms, 528; tied 249
Imagery, and absolute ear, 473 f.; auditory, 81, 86; concrete, 528, 531; and muscular tenseness, 85 ff.; and pleasantness, 530; synaesthetic, 528 ff.; and unpleasantness, 530; visual 80 ff., 99, 528 ff.
Imageless thought 145, 361, 370
Imagination 25 ff., 47, 55, 7 ff., 85, 318, 351
Imitation 29, 39
Immediate memory 615 ff.
Impression, method of 311
Impulse, 25, 29, 60, 306; gambling, 167; nervous, 55 f.; reduction of 76
Intelligence, 31 f., 54, 140, 150 f., 401 ff., 419 422, 425 ff., 434, 467; and crime, 40; and environment, 124 ff.; general, 62; and handwriting, 166; and instinct, 59; and nationality, 128; quantitative principles of, 143 ff.; and range of report, 442 ff., 467; and sex 139
Intelligence quotient, 126; and diagnosis, 146; and occupation, 135 ff., and speaking of English, 130; and school success 127
Inhibition, 36, 58, 61, 593, 602; central, peripheral, 42 ff., 461 ff.; Heymans' law 427 ff.
Innate ideas, 299; capacities 299
Innervation, diffuse, 602 ff.; sense 4
Insanity, inheritance of, 153; prevention of 153
Instinct, 28 f., 54, 59, 288, 291, 306; feelings, 28; gregarious 544
Instinctive reaction 59
Integration 154, 401 ff., 412
Intensity, 235, 427 ff., 461 ff.; of cognition and sentiency, 144; as criterion of movement 416 ff.
Interactionism 27
Interest, 38, 59, 108; natural 42
International Congress of Psychology, announcement of 468
Introspection, 12, 26, 47, 54 f., 60, 115, 118, 234 f., 287, 294, 306; experimental, 54; inadequacy of, 54; observation in 234

James-Lange theory 28, 121, 593
Journal of Applied Psychology 45
Judgment, 31, 39, 192 ff., 199 ff., 309 f.; of attributes, 469; immediate and mediate, 474, 488 ff.; of lifted weights, 564 ff.; of pitch, 469 ff.; relation between speed and accuracy in 483, 490 f.

Kinaesthesis, 235, 473, 489 ff., basis of judgment in lifted weights 569 ff.
Kinaesthetic sense, 168; cues in maze learning 549
Kinesimeter 12
Kingsbury Group Intelligence Test 138

Language ability, measurement of, 401 ff.; of children, 168; completion scale, 394 ff.; response 287, 294
Laughter 309
Law, bionomic, 25; of chance, 68; genetic 46
Laws, 17; lack of, in psychology, 514; parallel, of Cermak 510 ff.
Learning, 113, 150, 165, 342, 352; of sheep and goats in maze, 544 ff.; physical basis of, 371 ff.; process, 291; rate of, 480; and surface-tension, 389 ff.; transfer of 479 f.
Least observable interval 170
Lesion, 63; thalamic 595
Life, 20 f., 27, 57 f., 66 ff., 70; habits, 23; and mind, 27; stages of 41 ff.
Limen, 155; of fusion, 508; of memory 620
Listing's law 161
Localization, 54, 78, 514; auditory, tactile 476
Local sign 5, 13, 17
Logic, 19, 69, 142 f., 159, 318 f., 367, 450; Hegelian 22
Love, 28, 41 f., 245 f.; and memory 121

Magician among Spirits 148
Maze learning 544
Meaning, 54, 68, 100, 108, 155, 234, 236 ff., 345, 354, 360, 528, 539, 585; of odors, 531ff., auditory, in colors, 537; content, 541; error, 542; instruction, 413; problem of, in behaviorism, 233; as context, 235; as characteristic of behavior, 235; observable 237, 247 f.
Measurement, mental 145
Mechanical theory 69
Mechanism, 57 ff., 66 ff., 92, 234; and organism, 69; of consciousness, 588; of mental process 528
Mediums 148
Medulla 23
Mental age, 435; and vocabulary score 125
Mental atoms, 54; chemistry, 345, 354; combination, 33; deficiency, 165; development, 53; disorders, 64; decay, 168; element, 55, 74, 101; energy, 55, 56, 143; evolution, 22; growth, 168; heredity, 28, 49; measurement, 145; process, 6, 34, 55, 56; explanation of, 515; set, 95; unconscious, 330, 333, 337; systems, 59, 64; tests, 103; in hypnosis 226
Memory, 7, 69, 103 f., 108, 162, 210, 216, 297, 318, 342, 351, 354, 388, 391 f., 452, 456, 466, 504; muscular, 167; physiological, 170; primary 103 f.
Memory after-image 170
Memory and affective states, 112 ff.; and blood pressure, 116; and weather 109, 114
Memory-effect, 326, 338; factor, 328 f., fluctuation, 116 ff.; images, 103, 476; in hypnosis, 223; qualitative, 475; span, 224; determined by method of constant stimuli, 615; immediate, 615; methods of calculation, 615; limen of 620
Metaphysics 315, 451
Method, 49, 319; associative reaction, 88; objective, 26; observational, 26, 31; comparative, 26; experimental, 26; psychophysical, 63; of constant stimuli, 615; questionary, 26, 48; scoring in memory tests 610
Migraine 207 f., 212
Migratory impulse 167
Mind, 4 f., 21 ff., 43 f., 55 ff., 65, 72, 286, 368, 451; animal, 25 ff.; criteria of, 32; evolution of, 23; and body, 167, 289, 515; and brain, 27; and life, 27; as cause, 71; in use, 315 ff.; of primitive man 48
Miracles 167
Mood 108
Monocular vision 524
Moral disorders 64
Morality 42, 167
Morbidity 39, 42
Moscow Institute 156
Motion, 4 f., 13, 241; direction of 242
Motivation, 60 312
Motor coordination, 149; impulse, 10; reaction, 9; sense 13
Movement, 5, 12 f., 32, 74 f.; apparent, 446; emotional and expressive, 7; muscular, 79; perception of, 412, 498; theory of, 499; random, 36 f.; and emotion, 100; and extent, 513; and fusion, 510; and light mixture 510
Month form 530
Müller-Lyer figure, 54, 448; effect of, on movement 503, 506
Multiple personality 54
Muscle groups, 78; fibres, 60; sense 4, 77, 78
Muscles, accessory, 46, 49; fundamental, 47, 49; as organs of will, etc., 36; development of 36
Muscular action, 5, 61; contractions, 55; memory, 167; movement, 79 f.; in hypnosis, 279, 230 f.; sensation 84
Musical ability 479, 491

Nationality, and intelligence, 129, 136 ff.; and occupation 136 f.
Nativism 314
Nernst-Lillie theory 374 ff.
Nervousness, in sheep and goats 545 ff.
Neural scheme of attentive adjustment 602 ff.
Noise 108, 476
Nonsense syllables 75, 106, 114, 217, 324
Number forms, 167; in a synaesthetic subject 528

Object-consciousness 284
Observation, 234 f.; clinical 299; field, 299; self, 235; training 601
Observational task, influence of 326 ff

Occupation, and intelligence, 135 ff.; and nationality 136 f.
Octave errors 472 ff.
Odor, visualized 531
Oedipus complex 245
Olfactory perception, in a synaesthetic subject 531
Optic thalamus, 63; seat of pre-sensation 595
Order 23, 328 f.
Organism, 35, 60, 240 f., 287; behavior of, 55; living, 57 f.; non-living, 57; and mechanism, 69; as physical thing 236
Overtones and absolute pitch 471 f., 486

Pain, 28, 31 f., 41, 72, 78, 230; as pre-sensation 593 f.
Paired comparison 395 ff.
Parallelism 27, 301
Paranoia 154
Patellar reflex, and affective tone 553 ff.
Pattern, 69, 362 ff., 592; behavior, 247, 349, 494; of movement, 417; phenomenal, 494, 515; stimulus 515
Pedagogy, 7, 21, 36, 38; and psychology 21
Perception, 5, 7, 9, 32, 47, 53, 74, 100, 140, 216, 243 f., 297, 313, 315, 318, 340, 342, 346; in children, 42; collective, 350, 357; formal properties of, 494; inhibited, 591; of motion, 161; of movement, 412 f., 498 ff.; laws of, 503; space theories of, 52 ff.; new treatment of, 369; of odor in a synaesthetic subject, 531 ff.; of sound in a synaesthetic subject, 532 ff.; of space, 4 f., 168, 354, 359, 519 ff.; theories of 503, 506, 524 ff.
Performance tests 152
Periodicity, 41; of mental work 102; physiological 102
Personal equation 6, 108, 111, 168, 404
Perversions of youth 24, 38 f.
Phenomenology 303, 320, 323, 343 f., 353, 362, 515
Philosophy, 1 ff., 58, 66, 451; and psychology 321, 322
Phi-gamma hypothesis 155, 615, 627
Phi phenomenon, 412, 501 ff., 514; tactile, 513, 518; pure 502, 504
Photometry 171 ff., 178 ff.
Phototropism 234
Phylogenesis 25, 38, 292
Physics, 54, 233 ff., 292 f., 313 ff., 319, 506; and psychology 168, 365
Physiology, 54, 57, 62, 234, 293, 314; and psychology 313, 322 f., 316, 319, 451
Pitch, 532; absolute, 469 ff.; attributes of, 476; discrimination, 470; errors, 472; and quality, 475; relative 470
Play, 33, 38; tendencies 45
Pleasantness, and association reaction time, 560 f.; and imagery, 530 f., 536; and knee-jerk 556
Pleasure 28, 31 f., 38, 54, 454 ff.
Post-hypnotic suggestion 54
Practice, 6, 103 ff., 109, 206; in lifted weights 576 ff., 587
Pre-sensations 588 ff.
Pressure 15 ff., 221
Primary memory 103 ff.
Process attitude, 562 ff.; evolutionary, 47; instruction, 413; nervous and mental, 50; pattern 417
Processes, higher thought, 47; observable 351
Production-process 360, 506
Protopathic system 54
Protozoa, behavior of 237
Pseudofovea 348
Pseudo-hypnosis 216 ff.
Psychiatry 19, 293
Psychical research, 19, 64; in animal field 164, 169
Psychoanalysis 1, 154
Psychoneuroses 154
Psychonomic law 36, 51
Psychopathy 146
Psychophysical axiom, Müller's, 512; level, 518; law, 16; methods 16
Psychophysics, 451; rapid judgments in 576
Psychoses 37, 153
Psychotherapy 154
Pupillary reflex 371, 373, 381
Purpose 54, 57 ff., 67 ff.

Quality, 236, 250; as aid in absolute pitch, 474 f., 492; correlation of, with time-space factors, 512; and ground 497 f.
Questionary, 48; of popular beliefs, 419 ff.; studies 20

Reaction, 11, 34, 149, 163; experiment, 10, 313; instinctive, 59; learning of, 391; selective 602
Reactions, 49, 55; emotional, 424 ff.; secondary, 379; specificity of, in emotions, 424 ff.; visceral 244

Reaction time, 6, 8, 170, 217, 486; degree of clearness, 605 ff., 612; guilt 88 ff.
Reason, 42; pure and practical 45
Reasoning 31 f., 170
Recall, 112 ff.; of emotions, 456 ff.; speed of 457
Recognition, 53, 104, 353, 531; time of, 170; of tones 473, 476
Recollection 74
Re-education 149 f.
Reflex, 5, 36, 59, 162, 170, 380, 382; action, testing of, 167; arc, 590; inhibition, 61; excitation, 61; psychogalvanic, 424 ff.; patellar, and affective tone 553 ff.
Relations, 344 f., 355 ff.; education of, 141; real, ideal 141 f.
Relaxation, 73, 230; progressive, 76 ff., 85 ff.; and images, 80 ff.; technique of 76 ff.
Religion 44, 169
Report, fidelity of, 434 ff.; range of, 439; threshold, 444; oral and written compared 438, 444
Reproduction, 53, 59, 144; of digits 620
Reproductive tendency 345, 353
Repression 61
Response 55, 142 f., 293, 393; in fear, 100; neuromuscular, 149; overt 604
Retentiveness 167, 229, 388
Rhythm, 6, 11, 165 ff.; seasonal 103
Rivalry, dermal, 16; in vision 520
Russian Psychological Review 156

Sanford, E. C., 157 ff.; bibliography of 165
Saturation 188, 192, 194 ff., 198 ff., 204, 206, 284
Science 49, 67, 71, 291, 314
Scores, in memory tests 615 ff.
Seashore tests 477, 491
Seasonal rhythms 103
Self, 55, 60, 67, 240, 291; purposive 59
Self-consciousness 30, 57
Self-existence, struggle for 57 f.
Self-observation 235, 247
Senescence 22, 25 f., 34, 43
Sensation, 4 f., 8 f., 12, 16, 21, 32, 39, 53 ff., 62, 74, 77 f., 83, 85, 97 f., 161, 190 ff., 231, 235, 245 ff., 297, 343 ff., 347, 351, 360, 524, 585; and constancy hypothesis, 350 ff.; dermal, 13; form, 5; glassy, 249 ff., 284; as intensity and quality, 318, 352; motor, 12, 17; muscular, 484; organic, 98 ff.; origin of, 369 f.; rise of, 171 ff., 179, 187, 189 ff.; somatic, 78; synaesthetic, 529; transitional 352 f.
Sense-memory 170
Sense modalities 514
Sensitivity 7, 13 f., 15, 23, 168, 173, 177 ff., 188 ff., 216
Sensory components of fear, 97; intensity, 313; quality, 313; types 33
Sentence-completion 401 ff.
Sentience 140, 143
Sentiments 28
Set, 61, 352, 565; mental 95
Sex, 24, 35, 39, 41 ff., 46, 50, 59, 114; and handwriting, 166; and intelligence, 139; organic basis of 41
Sex-difference, 423; in learning a maze 547
Sexual desire 35
Shock, 153; effect of 382
Similarity 345, 354, 355
Skill 37, 166
Slate writing 148
Sleep, 85, 213, 217; sleep-walking 207
Sleight of hand 164
Smell 168
Somnambulism 207, 217, 231 f.
Soul, 5, 9 f., 22 ff., 26 ff., 30, 35, 38, 41, 44; collective 24
Space, 4 f., 32, 34, 70, 141, 313; empty, 250 f., form, 347; fourfold, 168; illusion of filled and unfilled, 11; monocular perception of, 168; tactual perception of, 17, 526; theories of, 524 ff.; and time 348, 513
Space error 564
Span, digit, 222; memory, 224, 615 ff.; of groups, 622; limen 620
Speech, figures of, in psychology 166
Speed of disc rotation and flicker 178 ff.
Spiritualism 148
Statistical limens, effect of attitude upon 562
Stereoscopic vision 519 ff.
Stimuli, inhibitory and active, 428 ff., 461 ff.; marginal 480, 482, 602
Stimulus, 4, 10, 12, 16, 46, 55, 60, 95 ff., 100, 114, 118 ff., 142, 149, 231, 241, 294, 348, 471, 474, 505; comparison of moving and stationary, 413 ff.; attitude, 415, 526 ff.; dis-

crete, 517; error, 77, 415, 495, 527 541; response, 287 f., 293; effect of, on consciousness 588 ff.
Strain 75
Stroboscopic movement 498 ff., 510
Structure, 145, 153, 292, 352, 359; of fear pattern, 100; function, 352; law of 355, 360
Structuralism 297
Subconscious 37
Subnormal and normal children compared 434 ff.
Suggestion 77, 214, 217 f., 232
Superior children 146
Superstition 418, 423
Surface color 496 f.
Surface tension and learning 389 ff.
Sympathy 42
Synaesthesia, 489, 527, 591, 594, 596; physiological theory of, 595 f.; a process of cognizing 539
Synaesthetic imagery, 528 ff., 533; patterns in music 534
Syphilis 147
System, 32, 49, 159, 348, 368; in psychology 52
Systolic pressure 114, 115

Taboo 35
Tactile space, 526; zero 16
Tactual reference 535
Taste 168
Teleology 48, 67 ff.
Telepathy 419, 422, 423
Temperament, 113, 152; tests of 454 ff.
Temperature, 64; and work, 105 f., 109, 111; spots 13
Tendencies, criminal, 24; play, 45; savage 45
Tests, 37, 103 ff., 105 ff.; *Aussage*, 434 ff.; Binet, 138; galvanometric, 455 f., 459 f.; mental, 103; performance, 152; scoring of, 597 ff.; Seashore 477
Thalamic lesions 595
Theories, chance, 349 f.; of movement, 499 ff.; relational 355
Thinking, 34; as play of muscles 75
Thought, 33 f., 53, 71, 297, 342, 344 f., 367, 370; higher, 47; imageless, 145; and muscular tenseness 85 ff.
Threshold 427 ff., 461 ff.
Thyroidectomy 544
Tickle 78
Tied image 249, 351
Timbre 471 f., 475, 489, 480
Time, 6, 13, 32, 34, 70 f., 141, 162, 328; association, 162, 166, 167; effect of, on spatial extents, 513; error, 564; illusion of filled and unfilled, 11; judgments, 165; sense 313
Tonal attributes 469, 475, 477
Tonality 475
Tonic effect 394
Touch 17, 32 f., 78, 165, 222
Trabue language scale 405
Transfer of ability, 478, 481; of learning 470, 481
Transposition 352
Trial and error 71, 343
Tridimensionality 283
Typewriting, skill in 166

Unconscious, direction of, 60; mind, 55; mental processes 55
Unpleasantness, 122 ff., 454 ff.; and association time, 560 ff.; and fear, 100; and knee-jerk, 556 ff., 561; and imagery 530, 536

Value 68 f., 345
Vernier chronoscope 164
Vision, 168, 170, 294, 429 ff.; binocular, 168, 170; distant, 83; facial 170
Visual threshold 433
Visualization 33, 166
Vitalism 12, 68, 234
Vividness 359, 601
Vocabulary score and mental age 125
Vocational adjustment, 166; guidance, 64; selection 64
Volition 54, 297
Volitional act 70
Voluntary control, 37; movement 162, 166

Ward, James, 449 ff.; psychology of 452
Weber's law 15, 58, 161, 170, 314
Weight, 15, 102, 165; of color, 192 ff., 198 ff., 202 ff.; estimation of, 15; perception and attitude 562
Will 11, 37, 69, 419, 422
Word association, 6; habit, 401 f.; memory 63
Work, 38, 64; and barometric changes, 105 f., 111; and humidity, 105 ff.; mental, 102; and memory, 109, 111; and temperature 105 f.
Würzburg school 344

Youth, 33, 39, 44; criminal, 40; of old age 43

Zöllner illusion 170, 506
Zoölogy 293

ISHIHARA'S TEST FOR COLOR BLINDNESS

12326.

So far, this is, we believe, the most practical and scientifically correct test devised. The majority of the tests used for this purpose are usually time-consuming in their application, difficult to interpret, too extensive for general use, at variance with the ordinary accepted classification of color blind individuals, defective in coloring, or possess some other feature or defect which makes them unsatisfactory for those engaged in clinical and research work.

This color blind test consists of 16 plates put up in book form. Each plate has one or two numbers, or a design in spots of one or more colors, on a background of spots of other colors. The coloring of the numbers and designs is such that the number or numbers seen by the color blind do not agree with those seen by the normal individual, while the designs are such that the color blind individual finds it impossible to trace them in their entirety.

The ingenuity, simplicity, correct coloring, and the clear cut results obtained with this test in practical work, make it, as stated in the American Journal of Physiological Optics, by Dr. J. H. Clark of the School of Hygiene and Public Health, Johns Hopkins University, the most successful of the various color blindness tests.....$4.50 net

PARSON'S MANOPTOSCOPE for Testing "HANDEDNESS"

12227.

A simple, inexpensive device for diagnosing the twin-phenomena "eyedness" and "handedness". It was designed by the author of "Lefthandedness", in which work he sets forth his discovery of the principle of "unilateral sighting" in binocular vision. Mr. Parson's book has reawakened the interest of anthropologists, psychologists, physiologists, ophthalmologists, physicians, teachers, and others in the subject of "handedness", as well as "eyedness", and we are glad to offer this simple device for experimenting along the new lines of research growing out of Mr. Parson's work.

The manoptoscope consists of a conical-shaped aluminium dark-box about 9 in. long, the wide end of which is placed against the face like a stereoscope. The accompanying target consists of two units—a cardboard exposing device and a sliding test chart.....$5.00 net

C. H. STOELTING CO.

MANUFACTURERS - PUBLISHERS - IMPORTERS - EXPORTERS

PSYCHOLOGICAL AND PHYSIOLOGICAL APPARATUS & SUPPLIES

418-428 N. HOMAN AVE. CHICAGO, ILL., U. S. A.

BY EDWARD BRADFORD TITCHENER

D.Sc., Ph.D., LL.D., Litt.D., Sage Professor of Psychology in the Graduate School, Lecturer on Psychology in the College of Arts and Sciences, Head of the Department of Psychology in Cornell University

An Outline of Psychology (first published 1896) 1921 ..... $2.25
A Primer of Psychology (1898) 1925 ..... 1.20
Experimental Psychology: A Manual of Laboratory Practice
I. Qualitative Experiments
Students' Manual (1901) 1919 ..... 2.60
Instructor's Manual (1901) 1918 ..... 3.50
II. Quantitative Experiments
Students' Manual (1905) 1915 ..... 2.60
Instructor's Manual (1905) 1915 ..... 3.50
Lectures on the Elementary Psychology of Feeling and Attention (1908) 1918 ..... 2.00
Lectures on the Experimental Psychology of the Thought-Processes (1909) 1.90
A Text-book of Psychology (1910) 1921 ..... 2.25
A Beginner's Psychology (1915) 1920 ..... 1.60
Systematic Psychology (*preparing*)

TRANSLATIONS

O. Külpe, Outlines of Psychology, Based upon the Results of Experimental Investigation (1895) $4.00
O. Külpe, Introduction to Philosophy (1897): translated by W. B. Pillsbury and E. B. Titchener 2.10
W. Wundt, Lectures on Human and Animal Psychology (1894): translated by J. E. Creighton and E. B. Titchener 3.50
W. Wundt, Ethics: An Investigation of the Facts and Laws of the Moral Life
Vol. I.: The Facts of the Moral Life (1897): translated by J. H. Gulliver and E. B. Titchener 2.50
Vol. II.: Ethical Systems (1897): translated by M. F. Washburn 2.50
Vol. III.: The Principles of Morality and the Departments of the Moral Life (1901): translated by M. F. Washburn 2.50
W. Wundt, Principles of Physiological Psychology
Vol. I.: Introduction: The Bodily Substrate of the Mental Life (1904) 4.00

THE MACMILLAN COMPANY
Publishers 60 Fifth Avenue New York